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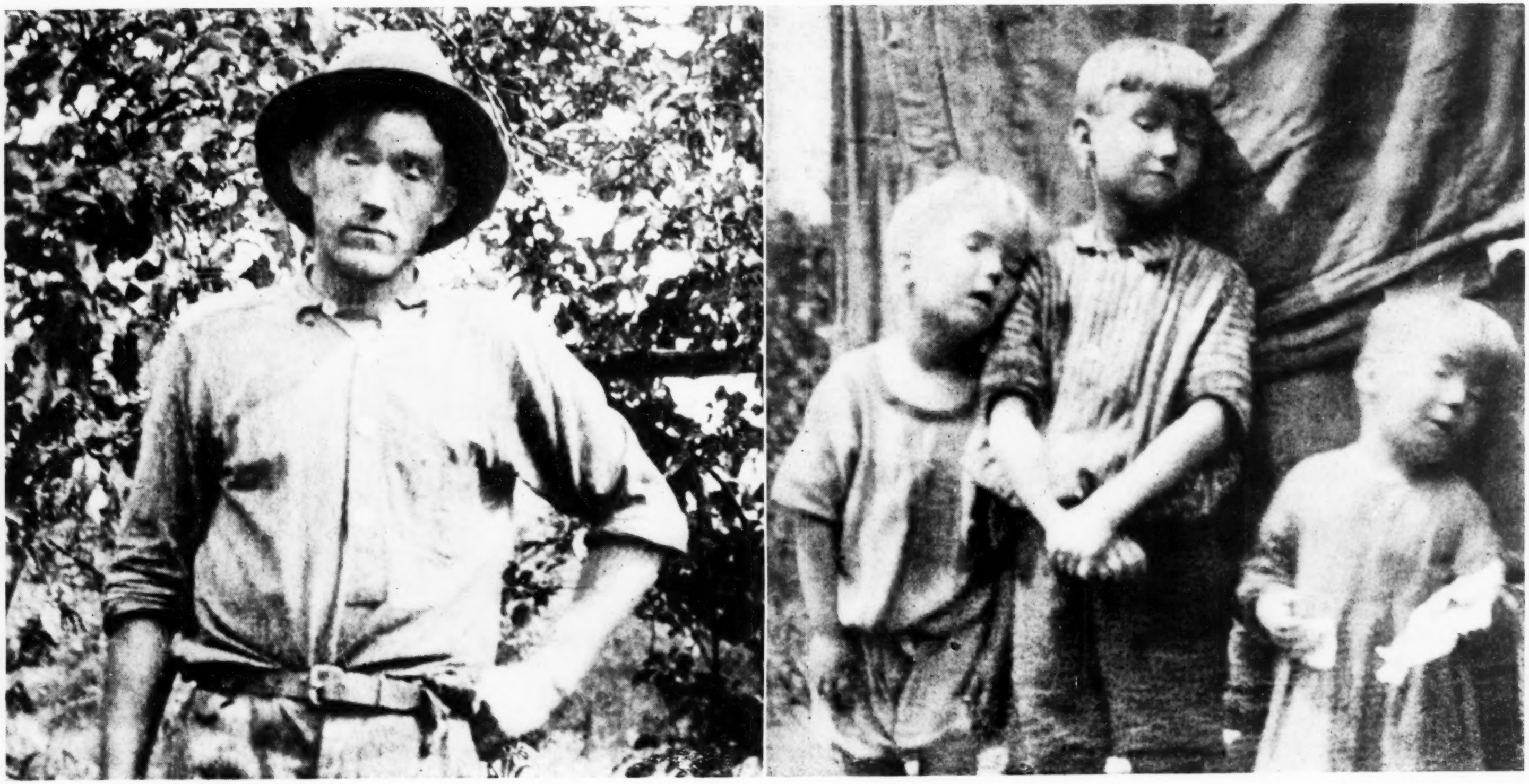

THESE CONSTRICTED EYELIDS RUN THROUGH FOUR GENERATIONS

Some, but not all, of the members of each generation are born with slit-eyes. The sight is normal, but the individual can see well only by throwing back the head or tilting it to one side, as the children are doing. The father (shown at the left in the above photograph) married a normal woman, and they have had four children. The three boys have eyes like the father's, as is shown by their photograph, above; a girl has normal eyes. (Frontispiece.)

THE SLIT-EYED PEOPLE

Constricted Eyelids Found in Four Generations of a Georgia Family—Vision is Normal—Defect is Not Sex-Linked and Might be a Mendelian Recessive

H. P. STUCKEY

Horticulturist, Georgia Experiment Station

WHILE in the mountains of northeastern Georgia last summer, I met a family which is interesting from the standpoint of the geneticist, because of the well-marked inheritance of a peculiar type of eye. One of the men of the family and three of his children are shown in the photograph opposite.

I saw a number of members of the family and made inquiries about others. The pioneer of the family in this section was the great-grandfather of the children whom I photographed, and I have the authority of residents of the locality for saying that he showed this restricted eyelid. Nothing is known about his past, so he must stand as the first individual in our family history.

Nothing being known of his wife, it must be assumed that she was normal. They had a son, whom I saw, and who is the grandfather of the children illustrated in the frontispiece. This man, now elderly, has the constricted eyelids well marked.

He married a woman with ordinary eyes, and they became the parents of nine children, six of whom had the slit-eyes, while three had eyes that were entirely normal. Among the children with the affected eyes are both boys and girls, so it is obvious that the defect cannot be inherited in a sex-linked fashion, as is color-blindness and one form of night-blindness.

These nine children form the third generation of the family history. One of them, the man shown in the frontispiece, married a normal woman, and they have four children, three boys (shown in the frontispiece) with constricted eyelids, and one girl who is quite normal.

The knowledge available therefore amounts to this—that the defect appears in some of the members of four generations, and that it is not limited to either sex. The history might give rise to a suspicion that the trait is a simple Mendelian recessive, but of course the fact could not be established with the data from only one family, and that a small one.

The defect is limited wholly to the lids. The eyesight is perfect, and the affected members of the family are able to earn their living in competition with normal individuals. Due to the constriction of the lids, however, the persons with this trait have difficulty in getting clear vision unless they throw their heads back or turn them to one side, as the children are doing in the picture.

Not being a medical man, I cannot tell the exact nature of this affection—it may be a failure of the nictitating membrane to be absorbed, in which case current theories of heredity would suggest either the absence of the factor for normal development, or the presence of an inhibitor for normal development. But such speculations are of little value when the data are so slender, and I do not offer any hypothesis as to the genetic nature of the case; I merely offer the facts and the photographs, which speak for themselves.

GERMINATING COCONUTS

Huge Seeds Have Remarkable Adaptations for Growing in Dry Climate—Cavity in Nut Serves Like a Stomach or Wet Nurse for the Young Plant—The Coconut not Naturally a Sea-shore Palm and Not Dispersed by Sea

O. F. Cook and C. B. Doyle

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COCONUTS are seeds. They are the largest of all seeds, except the so-called double coconuts of the Seychelles Islands, in the Indian Ocean, which are the seeds of a huge fan palm (*Lodoicea sechellarum*). The double coconuts are five or six times the size of true coconuts, weighing 30 to 40 pounds each. No other seed approaches the size of a large coconut, and few seeds, if any, afford such interesting specializations to assist in the germination and growth of the young plant. These special characters or adaptations are of the utmost interest as affording the best illustrations of the influence of selection upon the progress of evolution.

The germinating coconut has been studied in detail by several authors from the structural and chemical standpoints, but the remarkable internal growth of the embryo has not been adequately described and illustrated. Several varieties of coconuts sent from Panama were received in a germinating condition, so that it was possible to get photographs of the stages of germination as well as of the varietal differences.

THEORY OF DISTRIBUTION BY SEA

Appreciation of the specialized characters of the coconut has been hindered by the theory of maritime distribution. The coconut has been described very often in books of travel and natural history, and even in formal scientific works, as a plant that has been widely distributed in nature through the agency of ocean currents. It has always been considered a native of tropical sea-coasts, specially equipped for floating to other coasts and islands. It is true that the coconut is provided with a tough fibrous husk from one to two inches thick, which enables it to remain afloat for a long time. It is also provided with a coating of wax on the surface of the husk, as though to render the nut impervious to water. It is not surprising, therefore, that writers who approach the subject from the standpoint of structure alone should continue to rely on this apparently unimpeachable proof of the habits of the plant in nature.

There is no direct evidence, however, to support this theory, for the same type of husk is found in the seeds of many other related species of palms which do not grow on sea-coasts, and are not distributed by water. That the husk of the coconut is thicker than that of the other related species, merely conforms with its larger size and the greater danger of breakage when falling to the ground. The waxy coating of the husk is not a unique character, for it is found in nearly the whole group of palms, the small fruits of many inland species being more waxy than the coconut.

NEAR RELATIVES ARE AMERICAN

All of the palms that are closely related to the coconut are natives of America, most of them being found in Brazil. It is true that the coconut palm is most abundant and attains its greatest economic importance in the islands of the Pacific, but there is nothing to show that the habits of the palm would enable it to exist permanently, or in a truly wild state, in a littoral or oceanic environment. Although this idea of the coconut as a native of the Pacific islands appears in most of the text-books and general works of refer-

THE COCONUT ENCLOSED IN ITS HUSK

Sprouting coconuts from Panama, representing three varieties with different shapes. The sprouts show that the nut on left had germinated in an upright position; that on the right while lying on its side. (Reduced.) (Fig. 1.)

ence, it is entirely contrary to the opinion of writers who have been familiar with the actual conditions and behavior of the palm in the Pacific. Such writers agree that there are no wild palms, that the presence of coconuts is everywhere the work of man, and that the palms do not survive for any great period the withdrawal of human assistance.[1]

ADAPTATIONS FOR GERMINATION

The unique habit of the coconut of preserving a store of water in the interior cavity of the seed, the very large amount of food material stored in the "meat" of the nut, and the very thick, tough, fibrous husk are the most specialized features. Assuming that these peculiarities were necessary to assist in germination, it is reasonable to believe that the coconut palm must have developed in a relatively dry climate, where the young plants had to grow to large size before they could reach supplies of moisture in the soil.

Such extensive provisions for the storage of food and water would seem to be unnecessary in a maritime plant able to draw moisture from sandy beaches flooded twice a day by the tides. The distance to permanent moisture on a sandy beach is very short; in fact the sand is never really dry at all except at the surface. For a plant able to thrive on a salt solution, a covering of an inch or two of sand would be sufficient protection against drying out. The great size of the nuts would be a disadvantage for a seashore palm as keeping the seeds from being buried in the sand. The palms that live on the seashore, such as our southern palmettoes, all have small seeds.

It is only when we think of the coconut as growing in an interior region with an alkaline soil and subject to prolonged drought, that we can appreciate the significance of the large seed, or think of the large store of food and moisture, and the very thick husk as characters that give the palm special adaptation to the natural condition of environment. Many of the palms related to the coconut are forest species, or at least able to develop in partial shade. The coconut, on the contrary, is extremely intolerant of shade, and must have had its development in a region where other vegetation was absent or relatively sparse and open.

MOISTURE IN THE HUSK

The coconut is certainly well equipped to protect the embryo or the young seedling from the danger of drying. In addition to the moisture carried in the meat of the nut and in the central cavity, a still larger supply can be carried in the fibrous husk, and this may even be replenished from rains or other surface water after the young plant has begun to grow, and the husk opened and partly decayed. In addition to thus acting as a sponge, the husk serves the young plant in another way, as a medium for starting the growth of the roots before they enter the soil.

The whole process of germination may be completed inside the husk. And not the germination alone, but the subsequent growth of the young plant, may go on for months without any external contacts, the leaves often attaining considerable size before the roots have extended beyond the fibers of the husk.

A SELF-POTTED PLANT

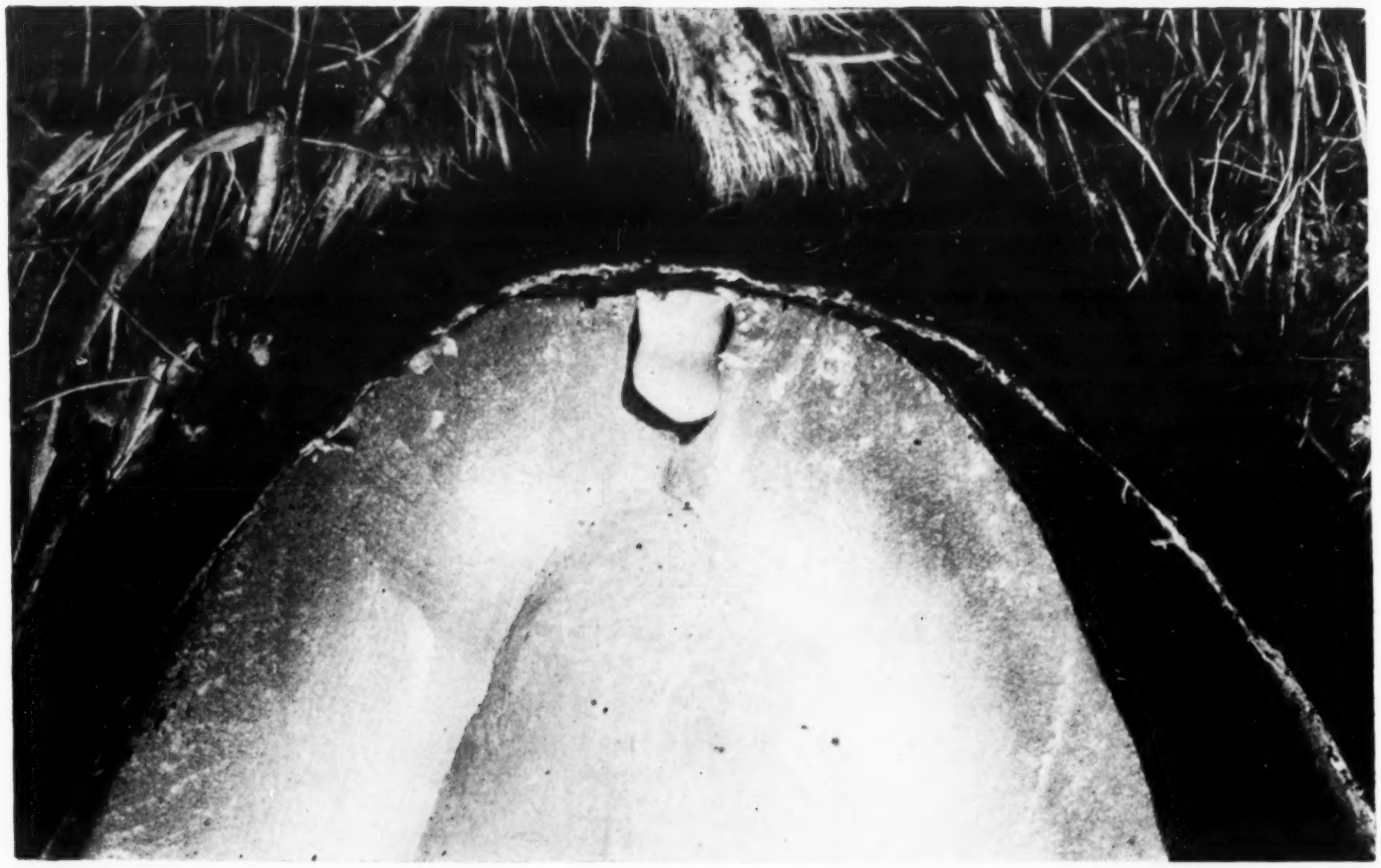
In the Philippine Islands where thousands of these palms are cultivated, it is often the custom to tie the nuts in pairs and hang them over a pole, fence, or around posts. Under these conditions the nuts will obviously receive no water from the soil, and it is apparent that they require no further attention, for in a few months the young plant has pushed its way through the husk and can be placed in its permanent location in the field.

The coconut seedling, with its ample provision for food and moisture and its fibrous husk for the accommodation

[1] Cook, O. F. The Origin and Distribution of the Coconut Palm. Contributions from the U. S. National Herbarium, 7:257–293, 1901. History of the Coconut Palm in America. Contribution from the U. S. National Herbarium, 14:271–342, 1910.

INTERNAL STRUCTURE OF THE COCONUT

Longitudinal sections of the nuts shown in fig. 1, representing three stages in the process of germination, indicated by the different sizes of the cotyledon or "nursing foot" that grows into the central cavity. The cotyledons differ in shape, that on the left having a more distinct shoulder near the point of attachment. Note also the relative size of the cotyledon and sprout in the central figure, the roots of which can be seen growing into the husk. (Reduced.) (Fig. 2.)



WHERE THE TREE GETS ITS START

This ungerminated coconut has been cut open lengthwise to show the embryo, which lies in a small cavity in the meat, at the stem end of the nut. When germination begins the embryo grows at both ends: outward to form the young plant and inward to fill the central cavity and digest the milk and endosperm, thereby supplying nourishment for the growth of the plant. Photograph magnified about 2½ times. (Fig. 3.)

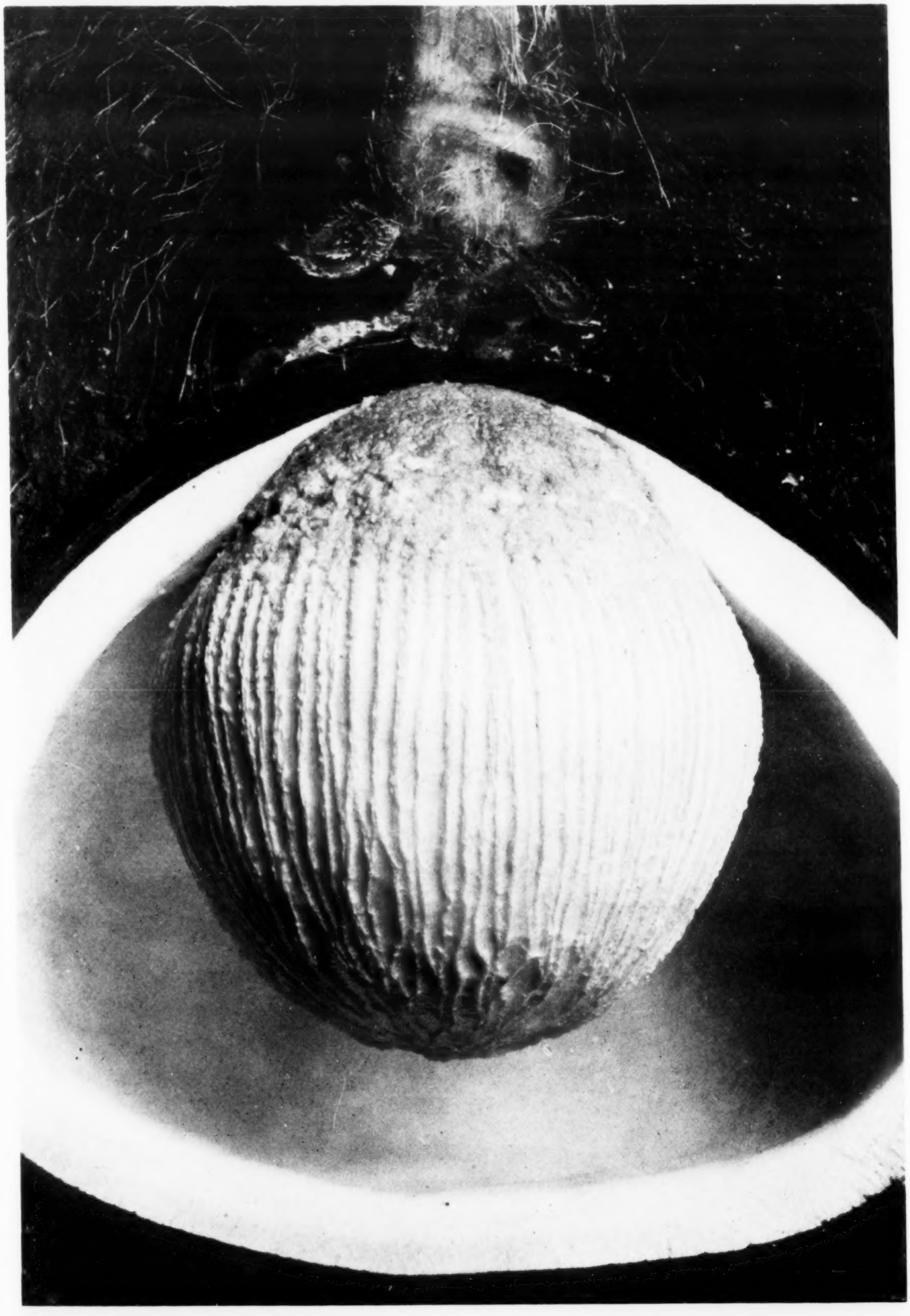
of its roots, may be described as a self-potted plant. So well adapted for this purpose is the coconut fiber that it is in general use by gardeners in Europe and America as a medium for the germination of delicate seeds, or as an ingredient of fine potting soil.

GROWTH OF THE EMBRYO

The use of the husk is still better appreciated when we take into account the remarkable processes that go on inside the shell of the germinating coconut. In spite of the huge size of the other parts of the seed, the embryo or young plant that exists before germination begins is hardly larger than the proverbial grain of mustard-seed. It is cylindrical in form and lies in a small cavity in the endosperm just underneath the largest of the three "eyes" of the shell. There is no direct connection between the embryo and the stored food material, or between the embryo and the "milk" in the central cavity. (See Fig. 3.) In order to utilize these food supplies, the embryo has to take them over by slow processes of digestion and absorption.

When germination begins the embryo elongates and becomes enlarged at both ends. From the outer end arise the plumule and the roots, while the internal growth results in the formation of a large bulbous mass of spongy tissue, pure white in color, with many grooves and narrow ridges running lengthwise on the surface.

All through this spongy mass, technically the cotyledon, are ramifications of vascular strands, which converge and become fibrous and woody at the narrow "neck" which connects the absorbing tissue with the stem. (See Fig. 6.) That the surface of some of these cotyledons is distinctly rougher and more irregular than others can be seen in Figs. 4 and 5. In Fig. 2 the wrinkles are narrow, parallel ridges, while Fig. 5 shows the wrinkles broader and distinctly irregular. Differences in the general shape of these organs are also apparent,



THE YOUNG COCONUT'S "WET NURSE"

Sprouted coconut with husk removed, showing a moderately advanced stage of germination. The cotyledon has swelled and its surface is covered with narrow, regular wrinkles. This surface takes up the "milk" of the nut and supplies it to the young plant above; and also digests the soft layer of "meat" or endosperm. Natural size. (Fig. 4.)

and may afford means of distinguishing some of the varieties.

The function of this cotyledon is to absorb the endosperm and carry the food material over to nourish the growing parts of the young seedling. In order to be absorbed, the food materials stored in the endosperm have to be digested, and the digestion is accomplished by ferments secreted by the cotyledon, as in the familiar change of starch into sugar in the malting of barley and other grains, preceding the formation of alcohol.

THE COCONUT'S STOMACH

The digestion of the endosperm is evidently more rapid at the places where it is in contact with the cotyledon. The softening of the surface elsewhere (see Fig. 5) may mean that the milk of the germinating nut becomes charged with a digestive ferment secreted by the cotyledon. Hence it may be considered that the fluid-filled cavity of the germinating nut, in addition to its storage function, serves like a stomach, to provide for a more rapid digestion and absorption of the stored food materials than would be possible by the direct action of the cotyledon. It is easy to understand that such a system might be very useful to the young plant in enabling it to make more rapid growth in short periods of favorable conditions. If this view is correct, we may think of the milk as being recharged with food materials from the endosperm, to replace those that are absorbed by the cotyledon.

That sugar is present in the cotyledon is evident from the sweet taste of the latter, and the very delicate texture suggesting sponge cake is altogether different from the hard oily "meat" of the nut. These enlarged cotyledons of the germinating nuts are considered luxuries and food for invalids by the natives of the Polynesian Islands and other parts of the tropics. In the native markets of towns along the west coast of Mexico the dried cotyledons are sold under the name of "manzanas de coco" or coconut apples. (See Fig. 6*b*.)

OTHER INTERNAL CHANGES

More detailed studies of what takes place inside the germinating coconut have shown interesting changes in the composition of the food materials. Thus Walker,[2] in his experiments with a selected series of nuts[3] in the Philippines, found that the total quantity of milk shows a marked diminution from 374 grams in an unsprouted nut to nothing when the sprouts had attained a height of 93 centimeters. A decided loss in the sugar content of the milk took place at the same time, falling from 2.3 per cent in the milk from the unsprouted nuts to 0.3 per cent in the ones which had sprouts 38 centimeters long.

A decided loss in the total weight of the meat was also evident, as it dropped from 457 grams in the unsprouted nut to 148 grams in the nut with a sprout 93 centimeters long. The loss seemed to be due to a direct absorption by the cotyledon, the process taking place only in that portion of the meat located near the latter, but increasing rapidly as the cotyledon grows larger and comes in contact with the entire surface of the endosperm. This would be the case, necessarily, if the nuts failed to maintain a supply of liquid in the cavity.

The loss in the total weight of oil was fairly proportional to the loss in the total weight of meat. During the early stages of germination there appeared to be a concentration of oil near the cotyledon, with a corresponding loss in that portion of the meat farthest away.

The percentage of sugar decreased from 4.1 per cent in the unsprouted nut to 1.2 per cent in the nut with the longest sprout (93 centimeters.) The loss appeared to be due to the absorption of sugar by the foot, as in all cases less sugar was found in that portion of the nut in direct contact with the cotyledon than in the parts farthest away from the cotyledon.

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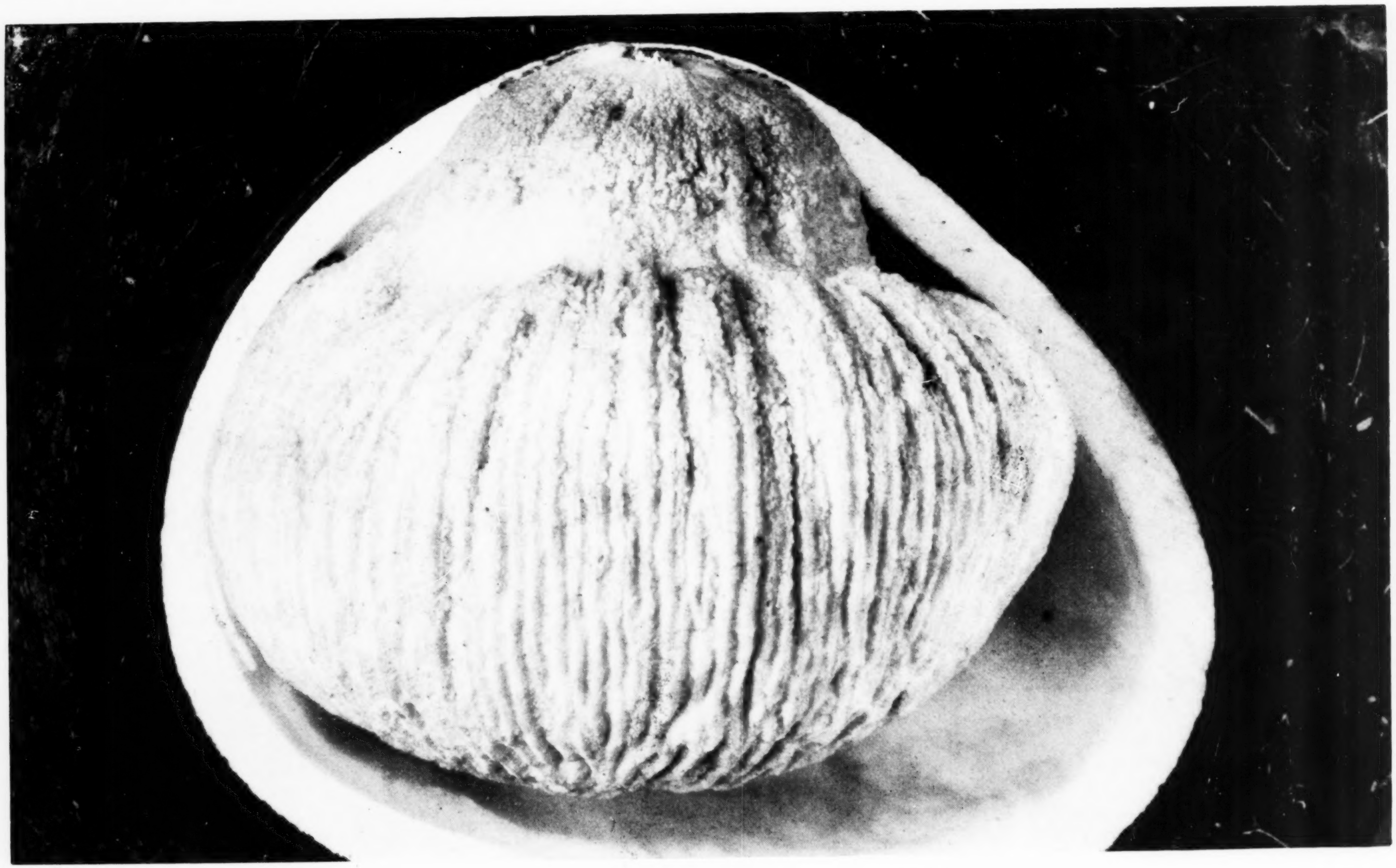
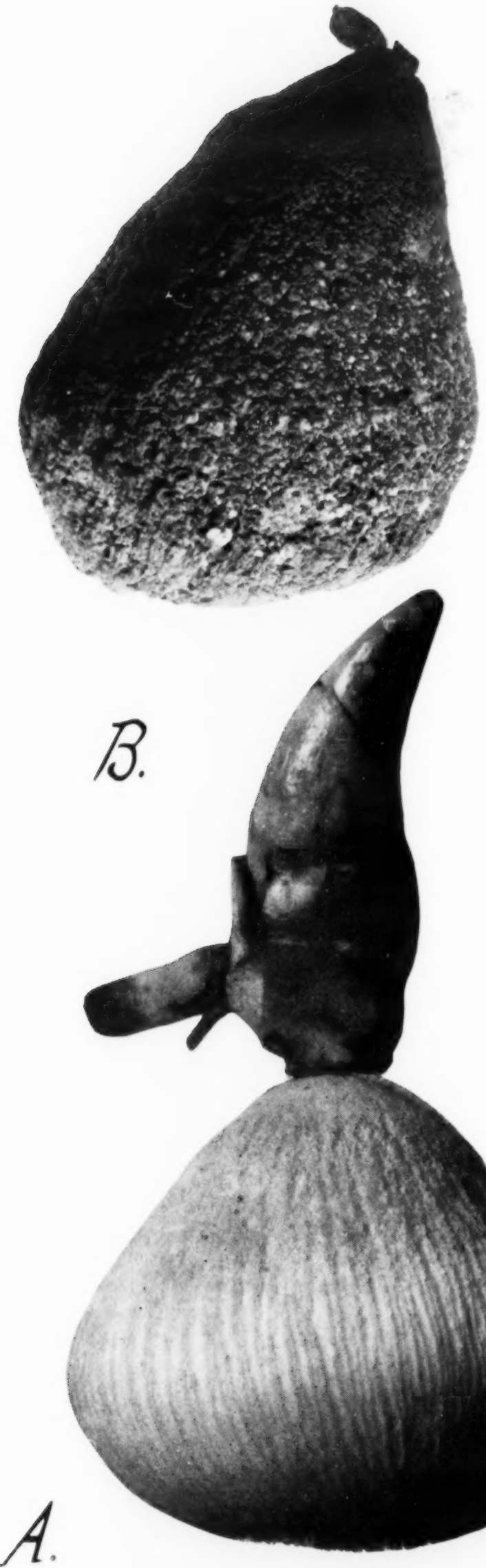
[2] Philippine Journal of Science, Vol. III, June, 1908.

[3] Four pairs of coconuts of different ages but approximately of the same size were selected for this work by Dr. Walker. The range in the age was from nuts which had not sprouted at all to nuts with a sprout 93 centimeters long.



SPROUTING COCONUT, FARTHER ADVANCED

The surface of the "nursing foot" or cotyledon becomes more corrugated as development proceeds. A soft, watery, partly digested layer of "meat" can be distinguished by its darker color, on the inner surface of the endosperm; this is gradually being digested to supply food to the young plant above. The supply of nourishment may continue for a long time after the roots have been sent into the ground. Permanent supplies of moisture must be reached before the survival of the young plant is assured but the "milk" gives it enough moisture for many months. Natural size. (Fig. 5.)



B.

A.

C.

THE SPROUT AND ITS FOOD SUPPLY

A. Coconut seedling removed from shell showing narrow "neck" between the cotyledon and the sprout through which the nourishment is carried. Natural size.

B. Dried coconut cotyledons called "coconut apples," (manzanas de coco) sold in the native markets along the west coast of Mexico. Natural size.

C. Dried coconut sprout, with the cotyledon removed showing the fibers that arise in the cotyledon and pass through the neck into the base of the young plant. These fibers serve no doubt to convey nourishment to the sprout, as well as to support the cotyledon at the point of attachment. Enlarged about 2½ diameters. (Fig. 6.)

The total weight of the cotyledon increased from 19 grams in the unsprouted nut to 228 grams in the nut with the longest sprout.

The cotyledon weighed 19 grams in a nut that still had no sprout, increasing to 288 grams in the nut with the longest sprout. With the growth of the cotyledon there was a decline in the percentage of sugar, although not in the total quantity, but more sugar was found at the stage when the foot completely filled the nut. It would be expected, however, that the amount of sugar present at any particular time would depend upon temperature, sunlight, moisture, or other conditions that would control the rate of growth of the young plant, or influence the process of digestion of the endosperm. With rapid growth the sugar would be used, while checking of growth would enable new supplies of sugar to be accumulated.

SUMMARY

The unique habit of the coconut of preserving a supply of water in the interior cavity of the seed, the very large amount of stored material or meat, and the very thick, tough, fibrous husk are features that afford an extremely interesting example of specialization to assist in the germination and growth of the young plant.

The cotyledons of the germinating nuts show considerable variation in shape, and also in the markings on the surface. Some of them have rather shallow, parallel ridges, while others are deeply and irregularly furrowed.

The formation of a soft, watery, superficial layer on the endosperm after germination begins indicates the presence of an active fat-splitting principle in the milk, which partly digests the meat and transforms it into such a condition that it can be readily absorbed by the growing cotyledon.

Eugenics Research in Bohemia

An institution for research in eugenics was founded in Prag, Bohemia, on July 12, 1913, by Drs. Karl Herfort and Arthur Brozek, who propose to make studies of the family history of school and hospital children in that city. Announcing their work in *Eos* (X, 3, 161–173, July, 1914) they report the preliminary study of fifty-six cases of feeble-mindedness, none of which was congenital. In almost all cases, however, the family stock was neuropathic. They conclude that from the marriage of two individuals, each of whom is neuropathic but neither of whom is feebleminded, one or more of the children is likely to be feebleminded. Discussing the Mendelian heredity of feeble-mindedness, they conclude it is not a unit character but that it is due to multiple factors. The neuropathic constitution, they conclude, is a complex made up of a large number of inherited dispositions; and as the different parts of this complex are inherited separately, there is naturally wide variation observed in the character of neuropathic individuals.

Encouragement for Superior Parents

Discussing the methods of constructive eugenics, leading to an increase in the number of highly gifted individuals in the race, Dr. von Gruber (in Archiv für Frauenkunde und Eugenetik, II, 1, 109) favors grants for education of children and pensions for parents. To give money to parents for the education of superior children would, he thinks, make such children regarded as an asset, and inferior children as a liability; not only would this lead parents to have large families, if they came of superior stock, but it would lead young people to regard the pedigrees of their prospective life-partners with more care, if they realized that intelligent choice in marriage was of distinct economic importance. The attention of citizens would thus come to be directed to the fact that superior children are of real value to the state and to their parents.

SUCCESS IN CONTROLLING SEX

Male-producing and Female-producing Eggs Regularly Distinct in Pigeons—Whitman-Riddle Experiments Result in Making Each Kind of Egg Produce Opposite Sex, Under Some Conditions—New View of Nature of Sex—Application to Human Society

WE TALK a good deal about the possibilities of sex-control—we who are interested in breeding. We would be much pleased if we could secure an excess of one sex, instead of the half-and-half which we usually get. If we could mate fowls in such a way as to produce *nothing but* males, or *nothing but* females, we would think we had reached the farthest goal conceivable—in that direction.

But an approximation to such a result has been obtained. Under exceptional circumstances, it is true—but given those circumstances, it is being done continually.

Professor C. O. Whitman, one of the greatest among the investigators and teachers of biology, who died in 1910, learned the basis upon which such matings may be made. After his death his work was continued by Dr. Oscar Riddle, who is getting the same results at the Carnegie Institution's laboratory for the study of experimental evolution, in Cold Spring Harbor, Long Island. Dr. Riddle, moreover, undertook, and is still engaged in, a special study of many aspects of these matings, in an attempt to decide whether a real, or only an apparent, sex control is involved. His answer has already been given—a reversal of sex has been effected. The results of these investigations are not yet published, but from the addresses, abstracts and short papers of Dr. Riddle, beginning in 1911, a fairly comprehensive survey of results may now be obtained.

To understand their results, let us first see what happens when we make a very *wide* cross in pigeons and doves, the material of their experiments. Ordinarily breeders mate members of the same species. Sometimes we can mate members of different species. Occasionally we can mate individuals of the next higher division, a genus; but we consider that a very wide cross. It is not often that we can go into the division above this, and mate members of two different families. But it can be done with pigeons.

Now when birds (not hybrids) of two different families are mated, the offspring produced are all males. In the whole history of the study there have been but two exceptions, and one at least of the females then produced was not perfect; the sexual development was rudimentary.

Here, then, we have a certain kind of sex-control, for we find that we can produce all males from a cross of two different families. So wide a cross as that hardly ever takes place in artificial breeding, and probably much less often or never in nature. But it suggests to us that if we are to get an insight into sex-determination, we must depart from the normal conditions as far as possible.

GENERIC CROSSES MADE

Dr. Riddle has found it most convenient, in view of Whitman's earlier results, to work with crosses of two different genera. From such a mating, the eggs produced in spring and early summer hatch into all or nearly all males. But if such a pair of birds are made to "overwork at reproduction," *i. e.* if they are not allowed to incubate their own eggs, and are made to lay eggs very rapidly, a pair each week or so, it will be found that as the season grows later, the eggs laid become progressively "weaker," until those near the end of the laying period will hatch with difficulty or not at all. From those eggs that do hatch from the birds which

become more and more exhausted, the proportion of females produced from their eggs becomes higher. Younger birds not previously "overworked" are not easily exhausted in this manner; but previously "overworked" old birds, under continued "overwork," will cease the production of male offspring earlier in the spring or summer than did these same birds during previous years; and from then on to the end of the egg-laying period, their eggs will hatch out all or nearly all females.

Now everyone knows that this is not what usually happens among birds. There are two unusual situations or elements involved in obtaining these results: first, a wide cross; and second, an enforced increase of "reproductive overwork." In ordinary matings of pigeons and of other fowls, the two sexes will hatch out in about equal numbers at any time of year. What is the meaning of this production, first of all males, later of all females, with the "overworked" pigeon in crosses of two different genera?

There are several conceivable explanations. First, it may be true sex-reversal—eggs which were destined to produce one sex may have been forced to produce the opposite sex. If that be the real explanation, we are going to get an insight into the nature of sex and the methods of controlling it. But it is also conceivable that nothing so sensational has happened. Perhaps male-producing and female-producing germs were formed in the usual manner, but for some reason only one kind was fertilized. Or perhaps one kind of germ died in the ovary, so that all the offspring had to be of the sex represented by the other kind of germ, which survived. Or perhaps there is a mechanical internal change in the female-producing germs which turned them into male-producing germs.

In this last paragraph is summed up the whole of the problem which faced Dr. Whitman and Dr. Riddle. To the biologists, it is a much bigger problem than it may appear to be on its face, for they know that ordinarily two kinds of germs are produced, one of which will give rise to males and the other to females, and it puts a very heavy strain on many accepted theories, to believe that one of these kinds could be made, *by pressure upon the germ during its growth period*, to produce contrary results to what the normal mechanism does. A change in the internal structure of the cell involving chromosomal change, or elimination, could perhaps, on current views, be accepted without great difficulty; but if Dr. Riddle demonstrates that he has changed the sex-value of the cell without a corresponding change or elimination of the chromosome numbers, he will find, as he is certainly well aware, that biologists here require very rigid proofs.

TEN LINES OF EVIDENCE

Ten different lines of evidence, or correlations with the breeding results, have been developed from the work of Dr. Whitman and Dr. Riddle, and they all seem to point the same way. To Dr. Riddle, only one conclusion is possible from them—namely, that sex has actually been reversed, that male offspring have been hatched from female-producing germs, and vice versa.

The first correlation established results from a study of the size of the ova, that is, the yolks freed from shell and albumen or "white." The yolks of late summer and autumn, those which produce all or mostly females, are distinctly larger than those of the early season, which produce males. The change in size is gradual, and considerable.

The pigeon regularly lays a clutch, two eggs, at an interval of a day or two apart. It was learned that the first egg of the clutch, in this experimental breeding of pure, non-hybrid females was rather regularly—there are exceptions—smaller than the second. Whitman had already shown that in the wild species with which he worked males predominate in hatches from the first eggs of clutches, and females from the second. So the conclusion was possible that males usually come from smaller eggs, both for season, and for egg of clutch; females from larger eggs, the larger of the season and the larger of the clutch.

Still a third situation was found to give evidence that the smaller yolks are male-producing and the larger yolks female-producing. It has already been mentioned that females which are "overworked" tend, when older, to begin the production of females earlier and earlier in the season. Now a comparison of the size of yolks derived from younger and older birds has shown that those of younger (but mature) birds are smaller than those of older birds. This fact has been fully demonstrated.

We have noted three kinds of evidence that smaller yolks produce males, larger yolks females. There is still a fourth piece of proof. It is known that the very first egg of life, and the first egg produced after a long period of rest, more frequently produce a female than do the first eggs of succeeding pairs or clutches. Study of the actual sizes of such yolks has shown that there is a reversal in size of these first eggs, corresponding to the reversal in sex, so that here too the female sex is associated with the larger yolk. Yolk size was accurately determined in about 10,000 cases, and the association of smaller yolk with male producing germs, and larger yolk with female producing germs is well established. The conclusion is drawn that here two kinds of ova are produced, and the kinds may be identified by size differences.

Next, let us recall that in the pigeon (probably in all birds) two kinds of eggs are produced, but only one kind of sperm. There is breeding evidence, in addition to the evidence accumulated by Dr. Riddle, which would seem to establish this fact. The determination of sex therefore resides necessarily in the egg, and the sperm can be ruled out of this discussion.

THE HEART OF THE PROBLEM

We get back to the heart of the problem again. If there are two kinds of eggs—one which produces males, and the other which produces females—how is it that early in the season practically *only* males are produced, and later in the season *only* females? Is there only one kind of egg produced at each period? And is the kind reversed in the two periods?

Quite impossible, Dr. Riddle answers.[1] "We can easily demonstrate, by weighing or otherwise measuring them, that both kinds of eggs are produced throughout. What actually happens is that the 'generic cross' which produces all, or nearly all, males in the spring and all, or nearly all, females in the autumn, is utilizing in the spring a number of female-producing ova for the production of males, and in the later season is utilizing for the production of females ova one-half of which had initial inclination for the production of females." That this is true, is shown not only by weighing the eggs, but by a number of other lines of evidence to be cited later; and also "by the fact that if the same female which threw all males in the spring had been mated with another bird of her own kind, and made to lay eggs at a similar rate, then both males and females would certainly have appeared at all seasons." It is only because we are studying a cross of birds, each from a distinct genus, and applying the pressure of "overwork," that we are upsetting the normal conditions enough to see what is going on beneath the surface.

We have now excluded two possible explanations of the observed sex-control in pigeons. We have shown that it cannot be due to selective fertilization by the sperm—for there is only one kind of sperm. We have shown that it can not be due to selective elimination of ova in the ovary—for it is known that the two kinds of ova—male-producing and female-producing—are being laid all the time.

The next possible objection to the supposition that we are dealing with a real case of sex-control, would be a technical one, alleging that everything which happens might be explained as differential maturation. To this Dr. Riddle answers:

> The maturation would have to be definitely differential in (1) the elimination of an X chromosome during the spring from one-half

[1] In a paper read before the American Society of Naturalists, Columbus, Ohio, December 30, 1915.

the ova, and the retention of this same X in the eggs of identical chromosomal constitution in the autumn. (2) The elimination of a Y chromosome from the other half of the eggs laid during the autumn, and the retention of of all these same Y's in the homologous eggs laid in the spring. Alternative assumptions to (1) and (2) would be that the X's of eggs laid in the spring became Y-like *in the presence of the sperm from a wide cross*, and only then become so; and that the Y's of autumn eggs become *gradually*, in the presence of *any kind of sperm*, more X-like in quality. (3) That all other chromosomes than the sex-chromosomes must display no such thing as a seasonal preference for "staying" or "going," since every observable character of the hybrids betrays an inheritance from both parents. If any one can accept such an incredible hypothesis of chromosome behavior, he must also face this fact: these sex-controlled experiments produce *several grades of females*. Some are quite nearly males, although they lay eggs. Is it too hazardous to suggest that in one and the same egg the Y could hardly have "gone out" to allow the egg to develop into a female, and yet have "stayed in" to deliver the relative masculinity which we easily detect and measure in this same female? If sex is directly the creature of a sex chromosome, the sex situation found in some of my female doves requires that the male-producing chromosome be eliminated from, and retained in, the same egg. In the face of these facts, it is wholly absurd to postulate a differential maturation as a basis for the observed phenomena of these sex-series. A true reversal of sex has been effected, and the possibilities of its being apparent rather than real have been excluded.

Another line of evidence concerns "developmental energy" of the spring-to-autumn series of eggs of the doves. It has already been mentioned that under the conditions of these experiments the last few eggs of the season are weak, or fail altogether to hatch. These eggs are, however, the largest of the entire season. It will be seen that those germs that *store* most materials are developmentally the weakest germs.

When we measure the length of life of the birds hatched, we find that the smaller eggs of the season, and probably too the smaller eggs of the clutch, give rise to the longest-lived birds. These smaller eggs furnish therefore this additional evidence of greater developmental energy than is possessed by the larger eggs.

Several hundred chemical analyses—each of a single yolk—have shown that the size increase of the eggs is accomplished by actual increase of the various solids of the egg; and that the increase from spring to autumn is gradual; there is nowhere any sudden break or variation. In connection with the analyses it was found that the smaller, spring eggs (male-producers) contain more water than do the larger, fall eggs (female-producers), and a similar relation seems to hold between the smaller and larger eggs of the clutches, as between the larger and smaller of the season. That is to say, the higher water content accompanies the male-producing germ, but the experimental procedures during the season carry all the yolks gradually to a lower level, and then all produce females.

These various facts are taken by Dr. Riddle to mean that the male-producing egg has a higher metabolism than the female-producing egg. If so, anything which increased the metabolism of an egg, which gave it greater vigor, would tend to make it produce a male. Now it is well known that crossing increases the vigor of offspring. Cross-breeding is used by every stock breeder to produce vigorous animals, and even the farmer sows by preference, cross-bred maize, because it yields more sturdy plants; so if we suppose that the amount of vigor is in proportion to the width of the cross, we will understand a possible explanation of the fact cited at the beginning of this paper, that when two *distinct families* of pigeons are crossed a higher metabolism is at once established and the offspring are all males.

Another line of available evidence relates to the variation in weight of the birds themselves at different seasons. It is found that at the season when the female lays the largest eggs, she herself, and also her consort, are smallest in size.

It has further been found that the females hatched from smaller eggs are more masculine in their behavior than females from larger eggs of the same clutch, and that females hatched early in the season are more masculine than their full sisters hatched late in the season.

The conclusion reached from weighing yolks, and from yolk-analyses, is

confirmed by burning yolks in the "bomb calorimeter," and measuring the amount of heat liberated in burning the stored materials. This very accurate method of investigation gives results which confirm the results of breeding, showing that eggs which produce males differ quantitatively from eggs which produce females. The results show too that the storage capacity of the eggs increases gradually during the progress of the season. This storage is highest at the time "developmental energy" is lowest, and both these coincide with the female-producing period.

POST-MORTEM STUDIES

The tenth and last kind of evidence brought from these studies is gained by post-mortem examination of the reproductive glands. Many species of birds, it will be remembered, have normally only one ovary, the right ovary regularly failing to develop, or degenerating rapidly after beginning to develop. This is true of pigeons and doves. But it is found that females hatched under conditions which accentuate the femaleness—for example, late in the season following crowded egg-laying—frequently have the right ovary developed. From this Dr. Riddle concludes that "the same pressure which carries the eggs of spring from male-producing to female-producing levels, also carries the earlier female-producing level to another yet more feminine."

In short, Dr. Riddle thinks, from the many kinds of evidence here outlined, that the nature of sex lies in the nature of differences between *levels of metabolism*, that the two levels are normally associated with different amounts of chromatin, or different chromosome numbers. But he considers these differential amounts or aggregates of chromatin as merely a *means* of insuring two diverse metabolic levels, and thus the two sexes; and he asserts that "if a new metabolic level is forced upon the germ, as in our experiments, the sex of the resulting offspring must coincide with the sex that can develop from this level, and this quite regardless of whether the level was established through a differential chromatin relation or value, or by other means." . . . "Males arise from germs at higher levels, females from the lower."

If it is merely a matter of level, and not a matter, as many have supposed, of some mechanical difference in structure, it is obvious that one might sanely hope to reverse sex. All that would be needed would be to exert sufficient pressure of an appropriate sort. Normally the two different kinds of eggs remain at different levels; one regularly produces males, the other females. To exert the heavy pressure necessary, Dr. Whitman, and later Dr. Riddle, made wide crosses. The pressure thus exerted (speaking metaphorically, of course), is sufficient, while both birds are producing their "strongest germs," to force female-producing eggs to a male-producing level. But when the cross is not of more than generic value, and the birds are made to yield weaker and weaker germs through reproductive overwork, the earlier male-producing level is followed in the weaker germs by a lower female-producing level.

A SKEPTICAL AGE

Every year a number of individuals claim to have controlled or reversed sex. Consequently, the biological world has become decidedly skeptical on the subject. "The insufficiently controlled experiment, the novice and the quack are the trinity of evils that has begotten this widespread skepticism," says Dr. Riddle.

At least one good effect it has had—any experiment which professes to show a control of sex is submitted to an extraordinarily stringent examination. Very few of the experiments stand this test, but there are a few, made by biologists of repute, which seem to have withstood criticism successfully, though in all, or in nearly all of these, it has been impossible to disprove one or another of the possibilities urged by the "chromosomists." Most of these publications have appeared since Dr. Riddle's first short statement[2] in 1911

[2] Paper read December, 1911, before the American Society of Zoologists at Princeton; abstract in *Science*, N.S., Vol. 35, pp. 462–463, March 22, 1912.

of the results with pigeons. These and two or three earlier experimental investigations have been cited in a more recent publication by Dr. Riddle as having a common basis of agreement with his results, and with his theory of the causation of sex.

The German biologist Richard Hertwig and his pupils have succeeded in producing an excess of males from frog's eggs, by allowing these eggs to become "overripe" (take up water) before they were fertilized. Dr. Helen Dean King at the Wistar Institute, Philadelphia, has done just the opposite. By drying toads' eggs before they were fertilized she secured as high as 90% females.

The results agree with those of Dr. Riddle, when it is remembered that he found male-producing pigeon eggs contained more water than did the female-producing eggs. Hertwig increased the water content of frogs' eggs, and produced males; while females were produced when Miss King decreased the water content of toads' eggs.

Sex appears to have been controlled by Whitney in one of the lowest worms, and by Woltereck with a small crustacean. Finally, there are some observations on cattle, and Dr. Alexander Graham Bell's work[3] with sheep, which Dr. Riddle interprets in the light of his own work.

The most notable thing about the studies that have been made on the doves and pigeons, and the thing that distinguishes these studies from practically all others that lead in this direction, is to be found in the systematic attempt to decide, in this most favorable material, whether the sex-control is real or apparent. We have already indicated the results of the inquiry.

SOCIAL APPLICATIONS

The many theories about sex-control in man are usually based on observations in other animals, but as some of the essential facts in man are not known, all attempts at sex-determination at present are futile. But if Dr. Riddle's work withstands the searching examination which it is sure to receive as soon as it is published in full, and if it is agreed that sex is a plastic thing which can be changed by sufficient pressure (again speaking figuratively), then it would appear that sex-control in man is not so impossible as it has sometimes been thought to be in recent years.

Apart from this very obvious application of a knowledge of sex-control in human society, the new idea of the nature of sex opens up some interesting possibilities to the eugenist. If the Whitman-Riddle observations on pigeons should be found substantially to hold good for man, we would be in the way of understanding the existence of so many masculine women and effeminate men in the world—the men and women who make up that "intermediate sex" of which much has been heard lately.

Sex-conservation, perhaps, will be one of the future planks in the eugenics platform. As Dr. Riddle once pointed out, there are in this country probably more masculine women than feeble-minded individuals, and more effeminate men than criminals. From a biological point of view, they are usually regarded as undesirable. "At present we look upon the appearance of the inadequately sexed individual as inevitable; just as a generation ago we looked upon the presence of the feeble-minded as inevitable. But once we realize that sex—its kind and quantity—can be controlled, we are brought face to face with many new possibilities, and some new responsibilities, in this direction."[4]

If the amount of sex possessed by a man or woman is partly dependent on the influences which surround the individual—on the environment, in short—perhaps we are making a mistake by throwing men and women into environments which are constantly becoming

[3] See the JOURNAL OF HEREDITY, Vol. V, No. 2, pp. 47–57, February, 1914. Some of the other cases are described by T. H. Morgan in his book on Heredity and Sex (New York, 1913).

[4] This and the succeeding quotations are from Dr. Riddle's paper on "The Determination of Sex and Its Experimental Control;" in *Bull. of the American Academy of Medicine*, Vol. XV, No. 5, October, 1914. Some of the material was also published in the *Journal of the Nat. Inst. Soc. Sci.*, Vol. I, No. 1, pp. 39–42; December, 1915.

more similar, and into activities which are growing ever more parallel.

The idea of the biological equality of the two sexes has, in Dr. Riddle's view, been taken altogether too much for granted in modern civilization. "However definitely an equality may exist from social, political or ethical points of view, it is doubtful whether this can be truthfully asserted from any biological standpoint." Man and woman differ in every cell of the body. The differences are numerous, and the whole problem complicated. Those who think they have solved it by laying down a "fundamental equality" of the two sexes may conceivably be considerably disturbed by biological progress in the future.

There is one other outlook which Dr. Riddle thinks his work opens up to the eugenist.

NEW HOPE FOR EUGENICS.

"You well know," he says, "that eugenics in our day lays chief stress upon *heredity*—upon the transmission, intact and unchanged—from parent to offspring of weakness or of strength, of fitness or unfitness, of the manifold characteristics of the organism. And the chief remedy suggested rests upon an elimination of the bearers of weak or unfit germs from the citizenship permitted to leave offspring. And it is of course wholly right that the emphasis now be placed on heredity since it is the ready practical instrument—the one that can be used, and indeed one that the race may never cease to use.

"But is there not a lot of fatalistic philosophy in the conception that mankind's exaltation and power require that mankind eliminate from all share in posterity the base and the weak? Shall man—a maker of environments—when confronted with the problems of his own improvement permanently and sadly turn to the crude and original methods of nature herself?

"At least to those biologists and men of medicine who believe that life-processes are controllable—developmental processes along with the rest—that conception and that remedy will not seem final. To those of us who realize that one characteristic, namely sex, has already been controlled, indicating that *in nature all are controllable* if our industry will but put light where our ignorance now enthrones mystery; to some of us, the production of strength from weakness, of more fit from the less fit, and better from the best, will seem more in keeping with the present general aim of our science, which is to secure control over all life-processes."

Course of Lectures on Eugenics

The Young Men's Christian Association of Washington, D. C., is presenting a public course of free lectures on eugenics under the direction of Paul Popenoe, of the American Genetic Association. The speakers are as follows: March 14, Paul Popenoe, "Prenatal Influences;" March 21, Prof. Roswell H. Johnson, of the University of Pittsburgh, "What Feminism May Do;" March 28, Alexander Johnson, Field Secretary, Committee on Provision for the Feebleminded (Philadelphia), "Bad Breeding in Washington;" April 4, Paul Popenoe, "Laws of Heredity in Man;" April 11, A. E. Hamilton, New York, "What One Baby Did for Race Betterment;" April 18, A. E. Hamilton, "The Gist of Eugenics;" April 25, Dr. Alexander Graham Bell will close the course with an address containing the unpublished results of some of his recent research in heredity. A similar course of lectures was presented last year, with the cooperation of the American Genetic Association, and proved highly successful.

SPOTTED ASSES

An Animal That, Like the Camel and Elephant, Rarely Has Spots—Piebalds More Common in Other Domesticated Animals—Selective Breeding Probably Largely Responsible for This Albinism

ALBERT ERNEST JENKS
Professor of Anthropology, University of Minnesota, Minneapolis, Minn.

FOUR years ago when carrying on investigations of the hereditary spotting of human skin I one day chanced to recall that many individual members of the species of domestic and pet animals and fowls with which I am most familiar at home are spotted with white. The wild members of domestic species and the wild species most nearly akin to our domestic animals are typically or specifically marked. Seldom is this specific marking spotted white. A little time spent in the library showed that the spotting of most of the members of the species of domestic and pet animals is non-specific, and showed that all species of domestic and pet animals spot, except the elephants, camels, and asses (*Equus asinus*.) Further research revealed spotted individuals among the Asian elephants (*Elephas indicus*), and the African camels (*Camelus dromedarius*). Nowhere could I find record of spotted asses.

At that stage of my interest in the question of the spotting of animals and men I made a trip to California and Arizona, and returned *via* New Mexico and Texas. There are many thousands of asses (locally called burros) in those States. I inquired of a score of persons long resident in the southwest, but found no one who recalled ever having seen a burro marked with white spots. I spent eight weeks during January, February, and March, 1914, in Arizona from near the Mexican border to near the central part of the State. No one of whom I inquired had ever noticed a spotted burro. I began to think that the ass should be added to the traditional leopard as a conservative in skin pattern.

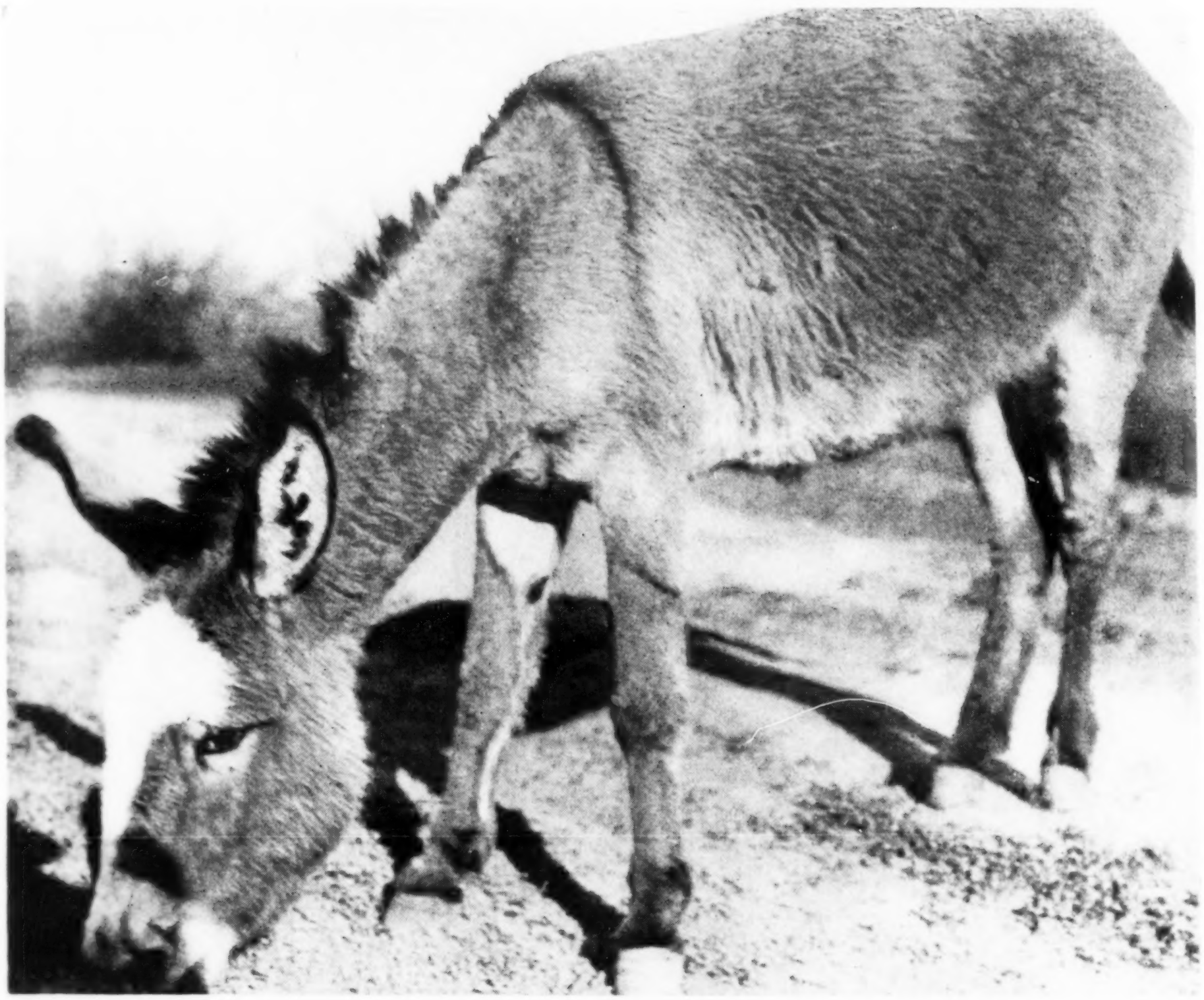
March 10, 1914, while driving immediately south of Tucson, Arizona, the burro shown in Fig. 7 was discovered. She was a small fawn-colored jenny with an irregular "blaze" face. She seemed determined to finish her feeding by the roadside, but was finally started toward home. She settled down "at home" in a small woven-wire corral with two other small jennys, one of which had a colt. The burros were in the possession of a group of Mexicans who had recently drifted northward from the State of Sonora, Mexico (a distance of about sixty miles), because of the revolution there. They reported they had brought the spotted burro from near the Mexican border where it had been sired by a jack which "looked just like it." The mother of the blaze-faced animal is light brown with the usual light colored muzzle and belly areas.

March 14, 1914, I passed by train through Charleston, Arizona, where there were three large brown burros feeding near the station. One of them had a blaze-face, and its right fore foot was also white to well above the ankle. The next day near Marfa, Texas, I saw two other white-faced brown burros. After that I felt that there was nothing new under the sun—even in the markings of burros.

IN MEDITERRANEAN LANDS

April and May, 1914, were spent in north Africa in Algeria and Tunisia, and in Sicily. In those countries I saw literally thousands of asses or donkeys. There were the usual ones of light fawn color, the browns, the blacks, the ash-grays, and a few which were white[1], but

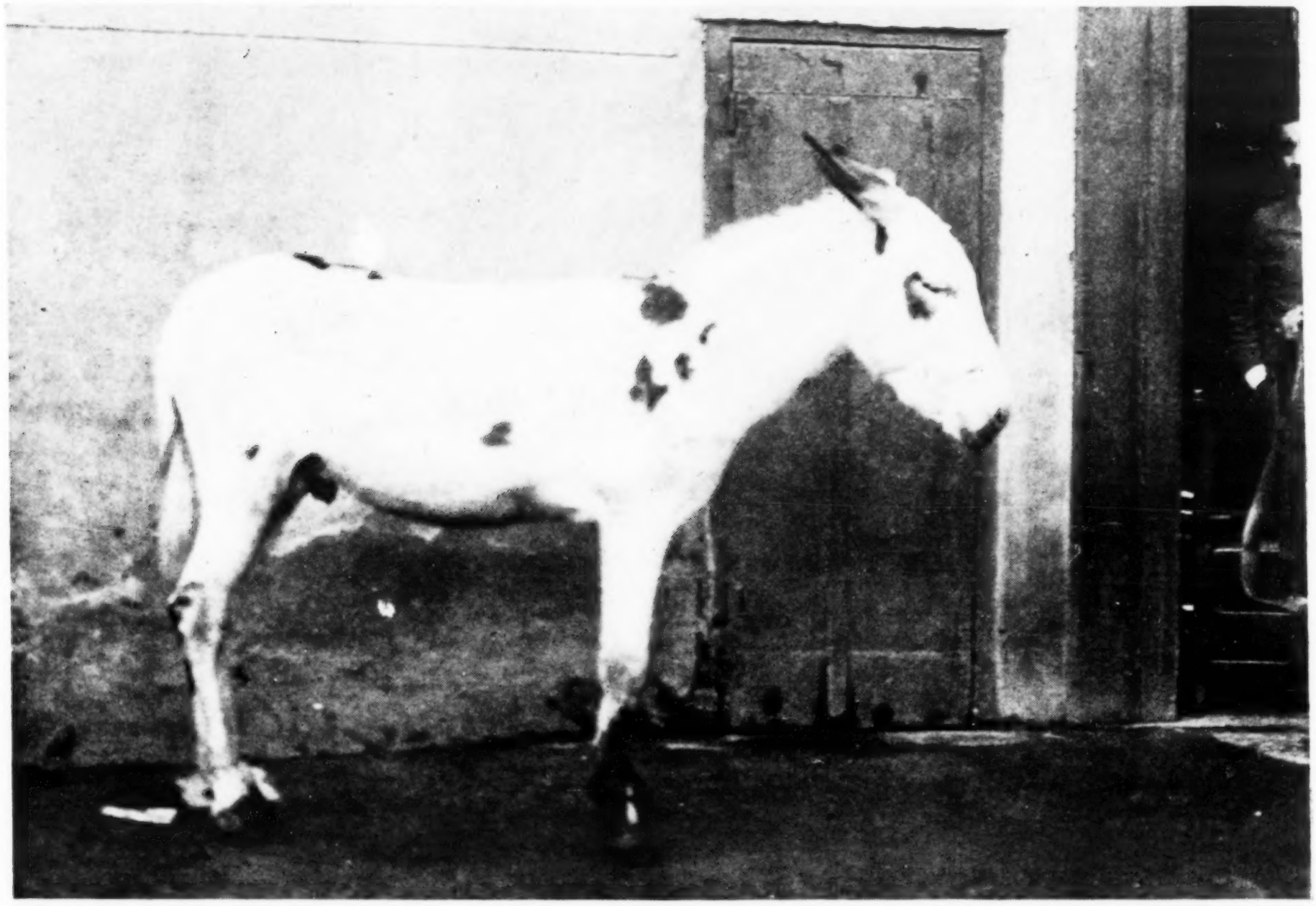
[1] Lydekker speaks of the Damascus breed of asses as frequently exhibiting white animals. —*The Horse and Its Relatives*, p. 222.



A "BLAZE" FACE BURRO IN ARIZONA

Spotted horses, dogs, cows are common enough, but did you ever see a spotted ass? It is perhaps the rarest of the spotted domesticated animals, except man himself. One of the reasons for this may be that the ass has not been subjected to such severe selection in breeding as have most domesticated animals, and spotting or partial albinism seems to develop under stringent selection, while it is extremely rare in animals living under wild conditions. Photograph by Albert Ernest Jenks. (Fig. 7.)

I neither saw spotted ones nor spoke with men who had noted them. Near the close of May we were in Naples. One day a beautiful, spotted donkey, spirited and high-stepping like a hackney, dashed past me around the corner of Piazza del Plebiscito. He was probably fourteen hands high, and dragged a gentleman's stylish two-wheeled cart with unusual speed and action. He was white, except for dark (brown) ears, a nearly continuous dark dorsal line, and dark spots on the shoulder from four to six inches across. For ten days I kept my eyes open in Naples for that dashing donkey, but I never saw him again. Later, however, I saw three others of the same appearance, though these latter animals were somewhat smaller, and plodded along the roads as donkeys are supposed to travel. One was at Sorrento across the Bay from Naples. He is shown in Fig. 8. One of the other two was on the road from Amalfi to Cava, and the other was in the outskirts of Cava. All four of the spotted donkeys noted in or near Naples were, apparently from their markings, of the same breed. All were white with dark ears, dark dorsal stripe, and a few other dark areas arranged bi-laterally with noticeable symmetry. I was told by the owner of the spotted donkey photographed in Sorrento that his father had obtained it near Naples, probably a short distance to the north, and that in



SPOTTED DONKEY AT SORRENTO, ITALY

The leopard has spots and, according to tradition, does not change them; the ass, on the contrary, has not spots and appears very reluctant to acquire them. But here is an Italian specimen which is mostly spot, the original dark color being reduced to a few patches. His facial expression lends some support to a theory that has been advanced—that albinism is due to a lack of vigor. Photograph by Albert Ernest Jenks. (Fig. 8.)

that vicinity there were others of the same appearance.

It has seemed worth while to call attention to these two types of asses, the "blaze" face, and the white with bi-lateral dark areas, because it appears that asses spot much less frequently than other domestic animals, with the exception of the elephant and the camel.

SPOTTING AND DOMESTICATION

So far as I am able to learn there are few exceptions to the rule that the members of wild species are typically or specifically marked. There appears to be no exception to the rule that in domestication all have yielded individuals which are spotted with white. To what is this due? Is the seemingly more frequent spotting of domestic animals (including human) due to an upsetting of the normal process of pigment metabolism in the conditions of domestication? Is the seemingly more frequent spotting of domestic animals due to the protection in domestication which saves the spotted individuals from the destructive selective forces which prey upon the animals of a natural (or "wild") environment? No matter which one of these conditions is the cause of the frequent spotting of domestic animals, and granting that both may have contributed, selection in breeding or forced interbreeding, or both, have probably greatly aided production of spotted domestic animals.

Selection by man has saved the spotted animals (even perfecting breeds with well-fixed spotted pattern), while clothing has saved the spotted man by making selection against spotted individuals more largely impossible. My colleague, Dr. C. E. Johnson, Department of Animal Biology, University of Min-

nesota, calls my attention to the fact that the ass, camel, and elephant have been the subjects of selective breeding to a relatively low degree as compared with most of the other domestic animals. The number of "breeds" among them is very small. This fact lays strong emphasis on selective breeding as the important factor in the spotting of domestic animals. However, we have the cases of spotted humans which are certainly not due to selective breeding. In the case of the human I am obliged to believe that domestication with its frequent large demands on nervous energy plays an important part in the production of the albinotic spottings of the skin.[2]

[2] Short bibliography of piebald humans:

1. Hutchinson, Sir Jonathan, "On Paleogenetic Face-pattern in Acroteric Piebalds," pp. 1479–1481 of *The British Medical Journal*, vol. I, June 18, 1910.
2. Simpson, Q. I. (with W. E. Castle), "A Family of Spotted Negroes," pp. 50–56 of *American Naturalist*, January, 1913.
3. Pearson, Karl, (with E. Nettleship, C. H. Usher, and B. C. Lamb), *A Monograph on Albinism in Man, Atlas, Part* 2, (London, 1913.)
4. Stannus, Dr. Hugh, "Anomalies of Pigmentation Among Natives of Nyasaland; A Contribution to the Study of Albinism," pp. 333–365 of *Biometrika*, October, 1913.
5. Jenks, Albert Ernest, "A Piebald Family of White Americans," pp. 221–237 of *American Anthropologist* (N.S.), vol. XVI, No. 2, April-June, 1914.
6. Cockayne, E. A., M.D., "A Piebald Family," pp. 197–200 of *Biometrika*, November, 1914.

Bad Eyes and Bad Hearts

As the latest theory of heredity assumes that every inherited factor in the germ-plasm affects not one but many parts of the body, interest in searching for these parallel effects, these "correlated variations," is increased. J. Strebel finds an association between certain hereditary forms of eye-defect (ektopia, myopia) and a weak heart, and reports the discovery in the Archiv für Rassen- und Gesellschafts-Biologie (X, 4). Evidence is not sufficient to show how close the relation between the two facts is, or whether indeed it is really a matter of heredity, rather than of chance association. It should be easy to collect further cases to determine whether these hereditary defects of the eye are regularly accompanied by defects of the heart.

War Hurts Scientific Breeding Abroad

Intelligent live-stock breeding is going by the board in France and Germany, and production "regardless of consequences" is being insisted upon, according to the *Agricultural Gazette of Canada* (p. 206, March, 1916). The exportation of cows is said to have been prohibited and orders given that all cows must be bred, no matter whether in accordance with a scientific plan of herd- and breed-improvement. "This means," says the *Gazette*, "that the education of a century will be partly undone abroad, and a breed of worse than grades—mongrels— is likely to be created that will have to be regenerated." Probably the result will be that Europe will have a much smaller amount of purebred stock for export after the war, than before, and American breeders will therefore be thrown more upon their own resources.

WHY CHILDREN RUN AWAY

Nomadism a Racial Trait, Inherited in Connection with Sex—Evidence from the Lower Animals and from Children—A Study of the Inheritance of Temperament

Review of a book by CHARLES B. DAVENPORT

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WHEN a child runs away, the mother usually ascribes its truancy to mere "naughtiness," or bad companions, or maybe original sin.

But the child's motive, according to Dr. Charles B. Davenport,[1] is the same as that which makes its parents visit Europe, or its Uncle John start off on a fishing trip. The causes ascribed for these various actions are usually quite different from the real cause, which is the primitive racial instinct to wander—an instinct so deep-seated that it is found in lower animals as well as in man.

We all have it, presumably; but some of us have it so hedged around by conventionalities, and the habits of civilized society, that we never really feel the call of the wild, or the blind impulse to start off somewhere—no matter where. Others are so impelled by this instinct that only lock and key can keep them in one place.

"It is a familiar observation," Dr. Davenport begins, "that persons differ greatly in their capacity for remaining quiet and satisfied for a long period in one place. One occasionally meets a woman who, though living within 30 miles of a metropolis, has, in the 80 years of her life, been there only once. At the other extreme are the tramps and gypsies who travel constantly or with only slight intermissions, and many of whom have repeatedly visited all quarters of the globe." On the other hand, there are those who do not travel constantly but who, leading a settled life, occasionally are impelled to jump over the traces, to run away, sometimes in a trance or dazed condition.

In short, the impulse to break camp and "hit the trail" is a very widespread one. Dr. Davenport describes all its forms by one name—nomadism, and drives the point home by quoting James Russell Lowell: "The American is nomadic in religion, in ideas, in morals, and leaves his faith and opinions with as much indifference as the house in which he lives."

Most of us Americans will probably be disposed to plead guilty to nomadism, of the literal kind, at least. Our ancestors must have had a certain amount of it, or we would not be here. Americans probably represent a selection of the more nomadic individuals of Europe, and it is no shock to learn that we are all at heart nomadic. A tendency to wander is indeed characteristic of primitive man and most animals. It is one of the traits, Dr. Davenport says, which sharply sets us off from most plants—although the botanist might tell a different story.

But look at our poor relations, the anthropoid apes—they seldom sleep twice in the same place. Or take the birds—the migratory habits of some species are notable. Others, on the contrary, are permanent residents of a place, seldom traveling more than a few miles, particularly if they are ground birds on a small island.

This difference in the migratory habits of birds suggests to Dr. Daven-

[1] The Feebly Inhibited: Nomadism, or the wandering impulse, with special reference to heredity; Inheritance of temperament. Pp. 158, price $1.50. Washington, D. C. Published by the Carnegie Institution, 1915.

port that nomadism is not a matter of social influences or of reading travel advertisements. "Whether a species tends to travel far or tends to stay near its home depends upon its constitutional factors—its instincts. The differences between men in respect to these points are as truly specific as the differences between swallows and grouse, and are as truly due to differences in inherited instincts."

But, it will be objected, the strength of the wandering instinct in a man varies. Once I wanted to join the Navy in order to "see the world;" then I stayed twenty years on the same job, quite contented. This alleged instinct of nomadism therefore cannot be really a constitutional trait, or it would not be subject to such fluctuations.

On the contrary, Dr. Davenport answers, we find that even the birds do not migrate all the time—only twice a year. We would expect the instinct to show itself only periodically. And we have other good evidence that there really is an inherited trait of nomadism.

We have already mentioned that the great apes have this instinct; and the basal instincts of these animals are the same as those of man. Then consider the primitive peoples and their migratory habits. If we look around the world we are driven to conclude that a wandering tendency—an absence of fixed abode—is widespread over the globe. "Indeed, it might be said that fixity of abode is a relatively recent acquisition, as yet only found in certain peoples in which the sedentary habit is highly developed; and that, consequently, it is not to be wondered at if even in a non-nomadic people like most of the Chinese, the French, or the Swiss, the racial trait of nomadism should persist in certain families, or after having been eliminated, have crept in again."

The extraordinarily common tendency to run away which children show, as every mother knows to her sorrow, is another bit of evidence proving that nomadism is really an inborn trait. The early life of the child, it is assumed, repeats the early life of the race; therefore it is no surprise to us that children, "true to their function of revealing the past, sometimes almost as soon as they have acquired the upright habit of locomotion, as if intoxicated by 'out-of-doors,' start off and, by some inner impulse, go on and on with no idea of where or why, tempted by an open gate or by the instinct to follow a man or a vehicle, or as a just-hatched chick follows any moving thing."[2]

ADOLESCENT RUNAWAYS

There is one more place where we must look for evidence. At the adolescent period the instincts are more highly developed than at any other time of life. If there is any widespread instinct of nomadism, it ought to show itself then. So it does. Kline, who collected 501 cases of runaways, found the greatest number of them occurred at the age of 15 years, and plenty more at 13 or 14.

From such evidence, Dr. Davenport feels justified in concluding that nomadism is a racial trait, a matter of heredity. In modern America, which has lured to itself the restless and those in whom love of ancestral home is weak, we naturally expect to find many families showing the romadic trait; and he has collected the histories of 100 of these families, embracing 616 individuals, on which he bases the present study.

A glance over the array of cases shows immediately that most of the nomads are males. That is exactly what the reader would expect, no doubt. Not only is it easier for a man than a woman to ride the brake-beams, not only is it easier for a man than a woman to roam through a life of vagrancy and avoid arrest, not only is man's courage of the kind which faces more readily the dangers and inconveniences of such a life; but, one would say, the nature of the male sex is such, and the nature of the female sex is such, that we would expect a life of nomadism to be more congenial to a man than a woman. Man is the active, restless, energetic, aggressive animal; woman is the contrary. We express the same idea in very superior language, nowadays, by saying that

[2] Quoted by Davenport from G. Stanley Hall, Adolescence, Vol. II, p. 376; New York, 1904.

woman is relatively more anabolic and man katabolic. Whatever words one uses, the facts are indisputable; historically woman's place is in the home (we use the words without any political implication) and man's rôle is that of the hunter and fighter.

Under such conditions it is no surprise to us to find that there are more male than female nomads in Dr. Davenport's tables. If nomadism is really an instinct, we are quite prepared to find it associated with sex—a sex-limited trait. Nomadism might be considered as much a sex-limited trait as is a mustache.

But Dr. Davenport does not adopt this obvious explanation, apparently[3] because there *are* a good many women nomads. Disregarding the fact that even women have some hair on the face, and some women have a great deal, he thinks that nomadism cannot be a sex-limited trait, since we find so many women displaying it.

Now there are, it will be recalled, two ways in which an inherited character can be associated with sex. First, it may be sex-limited; that is, it may be due to the secretions of the male sex-glands. Second, it may be sex-linked, that means according to current theories, that the factor for this trait "just happened" to get in the same chromosome with the factor which determines sex. So sex and a sex-linked character have to go together, but they are not due to the same cause, nor is one the cause of the other; their association is merely a coincidence.

Evidently, nomadism is associated in some way with sex. As Dr. Davenport has ruled out the first and most obvious explanation—that it is sex-limited, he has only one other possibility[4] to explain the greater number of affected males. It must be a sex-linked trait, like color-blindness.

TEST OF THE HYPOTHESIS

Fortunately, it is easy to test the correctness of this hypothesis. Without going at length into the theory of sex-linkage, we may say that it demands at least one simple result in the present case: if Davenport's explanation is right, then matings where the father is nomadic and the mother neither nomadic nor of a nomadic family, must result in no nomadic offspring whatever. The boys cannot be nomads because they cannot inherit a sex-linked trait from their father, but only from their mother. As neither the mother, nor her family, in this case had it, she cannot transmit it to them. The girls cannot be nomadic in either case. In families where the father is a nomad and the mother stays at home supporting the children by taking in washing, or something of the sort, the children must all have a perfectly domestic disposition, according to Davenport's hypothesis; they will not run the streets at nights, or steal rides on freight cars, or go to sea, or take to the road, or do anything else that nomads do and well-ordered children do not.

We turn to Davenport's table 5, which lists the matings of this critical kind. It lists thirty-two boys and eighteen girls. Instead of no nomad boys and no nomad girls, of this number, we find sixteen nomad boys and five nomad girls.

The hypothesis does not hold good, and although Dr. Davenport makes an attempt to explain the discrepancy in several legitimate ways, the difference seems to the reviewer to be too big to

[3] Against the hypothesis that nomadism is essentially a male characteristic is, he says, "the fact that nomadism is by no means confined to the male sex; in certain matings, daughters as well as sons are nomadic. The distribution of the nomadic trait among the offspring is, then, a function of a particular type of mating." The critical mating to test this hypothesis would be that in which both parents are nomadic. All the children from such a mating, girls and boys alike, should be nomadic. But Davenport's table of matings of this type contains only four families. No sound conclusion can be drawn from such a small number; but even there, it is worth noting that he gets one non-nomadic child, where his hypothesis requires that there should be none whatever.

[4] That is, only one other possibility in heredity. Of course, it may be said that the difference is one of training, girls being kept at home with mother while boys are turned out to roam the streets with "the gang." Probably this is a real factor in bringing about a larger number of nomadic men than women, but Dr. Davenport does not discuss it.

be overlooked, particularly as similar difficulties are found in the tables of some of the other types of matings.

That nomadism is hereditary, Dr. Davenport's study goes far to prove. That it is hereditary as a sex-linked character, the study does not prove. Moreover, many of the facts cited by Dr. Davenport—for example, that the nomadic instinct fails late in life, just as the sexual instinct does—indicate that it is really a sex-limited rather than a sex-linked character; that its association with sex is not wholly a matter of accident, as he assumes that it is.

If there is anyone who has not answered to his own satisfaction the question Why Girls Leave Home, we can at least give him a clue. It is because they inherit some of the qualities properly belonging to their more unstable, restless and nomadic brothers.

INHERITANCE OF TEMPERAMENT

Following his study of nomadism, Dr. Davenport undertakes an analysis of the distribution in families of temperament or its expression in *mood*. This is a problem of great fundamental importance, and it is a pleasure to note that eugenists are showing a tendency nowadays to attack such problems, instead of concentrating all their attention on degenerate conditions or trivial traits of no concern to the race.

We all recognize a certain average of normal temperament, says the writer, and we also recognize that this may change to an increased activity and elated emotional tone, on the one hand, or to a decreased activity and lower emotional tone on the other. He embraces all moods in these two divisions: the "hyperkinetic state" is that marked by one or all of the following elements: destructiveness, exaltation, homicidal acts and threats, irritability, psychomotor excitement, and violence. The "hypokinetic state" is on the whole the opposite of this, marked by anxiousness, worry, fear, slow movement, and so on. Finally, there is often an alternation of these two states.

The existence of these two contrasted moods, and the fact that an individual often passes from the one to the other, will be recognized by every one. But an examination of family histories shows that in some families there is a prevailing tendency for the one condition, in other families for the other. Dr. Davenport undertakes the task of bringing under one general scheme the inheritance of these various types of mood, and evolves the following hypothesis:

> There is in the germplasm a factor, *E*, which induces the more or less periodic occurrence of an excited condition (or an exceptionally strong reactibility to exciting presentations) and its absence, *e*, which results in an absence of extreme excitability. There are also the factor, *C*, which makes for normal cheerfulness of mood, and its absence, *c*, which permits a more or less periodic depression. Moreover, these factors behave as though in different chromosomes, so that they are inherited independently of each other and may occur in any combination.

This hypothesis is tested on eighty-nine families, embracing 629 progeny, many discrepancies are ascribed to "imperfect dominance," and in conclusion Dr. Davenport says "it is morally certain" that the hypothesis is correct.

Even though the confession of a doubt be tantamount to a confession of immorality, the reviewer cannot accept the conclusion with as much confidence as Dr. Davenport does. It seems to him that the whole analysis of moods is open to attack from the psychologists, and that the formula used to explain the heredity would explain almost anything. In other words, the reviewer cannot help feeling that Dr. Davenport has made the case much simpler than it really is.

He presents data on the inheritance of temperament in twins and the inheritance of the suicidal impulse. Even though it may eventually be found that his analysis of the inheritance of mood is not exhaustive, he must be given credit for having attacked an interesting, complicated, and important problem, and for having shown that in our daily behavior, where the power of heredity would perhaps be little suspected, our reactions are largely due to the hereditary nature of our temperaments.

FINDING THE PREPOTENT SIRE

Only One in a Thousand, in the Guernsey Breed of Cattle, Is Likely to Bring Marked Improvement to the Breed—Advanced Register the Only Way of Discovering Him

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THE superiority of one parent or breed in determining the characteristics of its offspring is termed *prepotency*. The fact that marked prepotency occurs in only a few animals in every breed makes it an important consideration in animal breeding. Indeed, nearly all of the so-called families in the various breeds, and in fact some of the breeds, owe their origin to a male or female ancestor which was strongly prepotent in features recognizable and useful to man.

While prepotency is generally thought of in connection with some useful character, it is not necessarily an advantageous thing. It is conceivable that an animal could be prepotent in a way diametrically opposite to the end sought by the breeder. In such a case the sooner the breeder recognizes and eliminates such an animal from his herd the more quickly will the desired results be obtained. In using the term here, however, we shall have in mind only prepotency in the production of butterfat.

Inasmuch as the dairy industry must rest upon an economic foundation, and since heavy production of milk and butterfat is the basic factor in profitable dairying, it is just as important, and perhaps more important, that the sires of "boarders" be eliminated in cattle breeding as it is to eliminate the "boarders" themselves. This can best be accomplished by the development of methods for the recognition of the prepotent animals and strains in production in the various breeds and the general use of these, both in grading and in building up purebred herds. The following study of the Guernsey breed was made with the idea of discovering, if possible, those animals and strains which have had, and are likely in the future to wield, the largest influence on the breed so far as higher production is concerned.[1]

Prepotent animals are usually discovered through the performance of their offspring, sometimes long after they themselves have died or perhaps been slaughtered in the prime of their breeding career. Probably discovery of prepotent animals will constitute an essential feature of breeding in the future, but the advanced register, if utilized properly, will eliminate in a large measure much of the chance attendant upon breeding and impressive sires may be selected with a certainty heretofore impossible. There are, without a doubt, in the untested stock of the country many great producing sires and dams, but the progressive breeder will scarcely take the risk of selecting sires from these, especially when they are not related closely to a tested strain of proven merit.

INDIVIDUAL PREPOTENCY

There are doubtless many ways in which the data of the advanced register might be used to discover those animals which have been most powerful in influencing the high production of the breed. The common method is to regard the number of advanced registry offspring as the test. This is open to the following criticisms: (1) Some of the greatest animals may be used in

[1] The data for this paper were secured from the Herd Register and Guernsey Breeders' Journals of the American Guernsey Cattle Club.

small herds and therefore have relatively few get, whereas an inferior animal may be used in a large herd and have relatively many get. (2) The advanced registry standard is so low that with judicious feeding and care almost any animal can make the advanced registry requirements. Indeed, at the present time the average requirement for the advanced register is practically 125 pounds below the average performance of the cows in the advanced register. This relation of different classes as compared with requirements is indicated in the following table:

AVERAGE PRODUCTION COMPARED WITH ADVANCED REGISTRY REQUIREMENTS

Class	*Age*	*Average Fat Record*	*Average A. R. Requirements*
A	5 up	490.4	360.000
B	4½–5	480.3	350.875
C	4–4½	457.4	332.625
D	3½–4	439.9	314.375
E	3–3½	411.8	295.125
F	2½–3	400.1	276.875
G	2–2½	377.3	259.625
Average		436.74	312.771

It appears from the table that the number of advanced registry cows which a certain sire has may therefore more properly be considered an indication of opportunity, prolificacy, or business expediency, than a test of prepotency in production, since a breeder in the habit of testing would place more daughters of his herd sires in the advanced register than one who did not, and a prolific bull in a large herd which is being regularly tested, may have an exceptionally large number of daughters in the advanced register without really being a sire of great producers. It is probably true, however, that when one daughter of a sire makes an unusually large record, his other daughters attract attention and are therefore tested, and in such cases the number of advanced register daughters might bear some relation to his merit.

Another method of studying prepotency in any breed, which would seem to be very promising, would be to compare the daughters of various sires with their dams, and note the extent of increase. Here, however we find the data far too limited to give anything like conclusive results. Most of the older sires would be entirely excluded from such a list. When the advanced register is older, this method of study would be more feasible than at present.

At the present time what seems the best method is to set a higher hypothetical standard of excellence for the offspring and judge the great sires by this. There is no practical reason for applying a similar test to the dams because these will be discovered through the achievements of their male ancestors or male descent. Indeed, if a prepotent dam does not have a prepotent son or perhaps grandson, her influence on the breed as a whole becomes practically negligible, however superior an animal she herself may be. This is due, of course, to the relatively large number of progeny of the male as compared with the female.

In setting a higher standard we choose to select 600 pounds of butterfat produced by a mature cow as the basis for a discriminatory study. We select 600 pounds as a standard because it may be safely assumed that any cow making such a record possesses individual excellence as a producer; for it is exceedingly doubtful whether such a record could be made from an ordinary cow through any artifice of feeding and milking. Using the same scale of increase (which appears to be substantially correct) as is now used in the advanced register, the 600 pounds standard for the different ages would be as indicated in the table on page 175.

From that table it may easily be seen that 490.5 pounds of butterfat for a 2-year-old cow is considered a 600-pound record, since such a cow could in all probability make such a record

ADVANCE REGISTRY REQUIREMENTS AND 600 POUNDS STANDARD

Class	*Regular Requirement*	*600 pounds Standard*
A	360.00	600.00
B	341.75—360.00	581.75—600.00
C	323.50—341.75	563.50—581.75
D	305.25—323.50	545.25—563.50
E	285.00—305.25	527.00—545.25
F	268.75—285.00	508.75—527.00
G	250.50—268.75	490.50—508.75

upon re-entry when mature. In computing "the mature cow equivalent" for any cow it will be found simplest to subtract the individual entrance requirement for the cow in question, and add this to the actual record made. Thus, a certain cow at 2 years of age made 606 pounds of butterfat. The entrance requirement at this age is 250.5 pounds of butterfat. Now, according to above formula, we have $(360-250.5)+606=715.5$ pounds, or the theoretical production of this cow at 5 years and above.

If a certain sire happens to produce a single daughter with a 600-pound record, we do not have in this alone adequate proof of his breeding capacity. This may be due to the dam, or, perhaps, a fortuitous variation. If, however, he produces three or more such daughters, the suggestion of individual prepotency becomes very strong. With this thought in mind we have, in the table at the botton of this page, listed all sires which have produced three or more daughters with records the equivalent of 600 pounds.

From the table it will readily be seen that we have up to December, 1915, only thirty-two sires which have produced three or more "equivalent of 600 pounds" daughters. There may be many others which were capable of such a

ALL SIRES HAVING AT LEAST THREE "EQUIVALENT OF 600 POUNDS" DAUGHTERS

	Reg. No.	*Birth Date*	*No. of A. R. Daughters*	*No. of "600 lb." Daughters*
Imp. King of the May	9001	1903	17	14
Imp. May Rose King	8336	1901	24	12
Golden Noble II	1836	1905	23	11
Imp. Masher's Sequel	11462	1900	64	9
Imp. Gov. I of the Chene	10563	1904	21	9
Imp. Lord Mar	14359	1903	24	7
Imp. Galaxy's Sequel	16904	1904	38	6
Rinaldo	8917	1903	11	5
Stranford's Glenwood of Pinehurst	13609	1906	12	4
Beda's May King	11893	1907	5	4
Dolly Dimple's May King of L	12997	1907	5	4
Charmante's Rose King	11746	1906	8	4
King Masher	11084	1906	7	4
Jewel's Independence	10324	1905	9	4
Jethro Bass	11366	1906	8	4
Triple Champion	13067	1907	12	4
Ledyard Bay	11074	1905	10	4
Imp. Golden Secret of Lilyvale	10028	1904	10	4
Justinee's Sequel of the Preel	2119	....	17	4
Lavanton	11611	1905	5	3
Imp. Coras Gov. of Chilmark	8971	1903	28	3
Glenwood's Main Stay 16th	9384	1903	9	3
Glenwood's Reputation	7687	1901	15	3
Glenwoods's Stranford	9386	1903	15	3
Buckthorn	4781	1896	11	3
Masher	63	1904	27	3
Imp. Holden IV	12179	1906	5	3
Fernwood of Homestead	7448	1901	9	3
Guydette	3966	1894	5	3
Starlight's Excelsior	7992	1902	23	3
Glenwood Boy of Haddon	4605	1895	36	3
Imp. The Conqueror II	15323	1905	13	3

record but do not have it because (1) some were used in small or grade herds and had little opportunity; (2) some are still too young to have many of their offspring tested; (3) some were too old at the time the advanced registry system was inaugurated; (4) many breeders do not test, hence records of daughters are unknown; (5) some breeders secure better and some poorer records as a result of different methods of feeding, etc. However, it seems certain that, regardless of the fact that a few worthy sires might perchance be excluded for the above and other reasons, we do have here 32 proven sires whose offspring have increased and may be depended upon in the future to increase the average production of the breed.

Up to December, 1915, there was a total of 254 sires who had produced one or more "equivalent of 600-pound" daughters. In other words, 32 sires, or only 12.6% of the total number of sires of 600 pound daughters, have three or more such daughters. This indicates that sires of great producing cows are relatively few in number.

There were up to December, 1915, 417 cows having records the equivalent of 600 pounds. The 32 sires listed above, produced 156 of these 417 cows. Therefore, we note that 12.6% of the sires of "600-pound" cows produced 37.4% of those cows.

Restating these facts in tabular form, we have the following data:

222 sires produced 261 "equivalent of 600-pound" daughters, or 1.17 each.

32 sires produced 156 "equivalent of 600-pound" daughters, or 4.87 each.

Thus, the 32 sires above, were more than four times as prepotent as the remaining 222 sires of "600-pound" cows.

When we observe that these 32 sires are only .092% of the male animals registered in the American Guernsey Herd Books and 2.20% of the 1,454 sires of advanced register cows, the tremendous importance of the few strong sires, from the standpoint of improving the production of the breed, becomes quite apparent. Indeed, it can be safely assumed from the above facts that less than one out of every thousand of the registered male animals will have a marked tendency to lift the production of the breed above the 600 pound level.

STRAIN OR FAMILY PREPOTENCY

What has been said above refers more particularly to individual prepotency or the tendency of a sire to get producing daughters. It is clear that a knowledge of the prepotent strains can only be gained by a study of the ancestry and descent of our great producing animals. When we study the pedigrees of the thirty-two sires in the above list, a noteworthy fact presents itself, namely, that they may nearly all be placed in a few groups according as they trace directly to some notable ancestor.

These groups are as follows:

GROUP A

Related to May Rose II 8648 E. G. H. B.

Charmante's Rose King.
Golden Noble II.
Imp. King of the May.
Imp. May Rose King.
Imp. Golden Secret of Lilyvale.
Jethro Bass.
Dolly Dimple's May King of Langwater.
Lavaton.
Beda's May King.

GROUP B

Related to Masher R. G. A. S. 705 P. S.

Imp. Masher's Sequel.
Justinee's Sequel of the Preel.
Imp. Galaxy's Sequel.
Triple Champion
King Masher.
Charmante's Rose King.
Imp. Cora's Governor of Chilmark.

GROUP C

Related to Governor of the Chene R. G. A. S. 1297 P. S.

Imp. Governor I of the Chene.
Imp. Holden IV.
Imp. Cora's Governor of Chilmark.
King Masher.
Imp. The Conqueror II.

GROUP D

Related to Imp. Glenwood Girl 1693

Glenwood Main Stay 16th.
Glenwood Stranford.
Glenwood Reputation.
Glenwood Boy of Haddon.
Stranford's Glenwood of Pinehurst.

GROUP E

Related to Sheet Anchor 2934
- Ledyard Bay.
- Charmante's Rose King.
- Triple Champion.
- Glenwood's Reputation.
- Glenwood's Main Stay 16th.
- Beda's May King.

GROUP F

Related to Imp. Sir Champion 58
- Guydette.
- Rinaldo.
- Buckthorn.

GROUP G

Related to Imp. Fernwood Lily 1468
- Starlight's Excelsior.
- Fernwood of Homestead.

GROUP H

Miscellaneous
- Imp. Lord Mar.
- Jewell's Independence.
- Masher 63.

It is readily apparent that in the larger groups above we have the most important representatives of what may properly be regarded as the Guernsey *families*. Thus we have the "May Roses," the "Mashers," the "Governor of the Chenes," the "Glenwoods," and the "Sheet Anchors." Groups F and G, while perhaps not commonly recognized as families, if we judge from the animals listed and others that may soon have three "600-pound" daughters, may properly be so regarded. The miscellaneous sires might be regarded as representing potential families requiring only the intelligent and consecutive effort of breeders to establish them.

Breeders must depend upon intelligent selection for whatever breed improvement is to take place in the future. Intelligent selection presupposes on the part of the breeder: (1) An appreciation of individuality as related to performance and other desirable characters; (2) a knowledge of ancestry in relation to breeding capacity for performance, prolificacy, constitutional vigor, etc. It follows, therefore, that a knowledge of the prepotent strains in the various breeds, whether it be for performance, prolificacy, constitutional vigor or other characters, becomes an invaluable aid in selection.

There are three systems of breeding represented in the ancestry of the above listed sires, viz., outcrossing, inbreeding, and line breeding. From these pedigrees it would be difficult to adduce sufficient evidence to prove the superiority of any one system. Group F offers a good illustration of a strain probably made prepotent by close breeding, *i. e.*, line and inbreeding. The dam of Guydette was an inbred cow. Rinaldo is the son of Guydette from a closely related dam. The May Roses have also been in and line bred to a very considerable extent. Theoretically close breeding by rendering character more pure, ought to give a more prepotent strain, and this theory seems to hold true in practice for a number of notable sires. However, it must be observed that some of the best sires in the breed are outcrosses between several of the leading families as instanced in the case of Charmante's Rose King.

The Guernsey, in common with all pure bred cattle, is prepotent over the ordinary stock of the country. This is an important asset to the progressive farmer because he can, by the utilization of good purebred sires, render his herd in a few years nearly, if not in fact, the producing equivalent of a purebred herd. The writer has observed a large number of calves by Guernsey sires, born to very ordinary cows of mixed breeding and in nearly every case the offspring showed very definitely the outstanding Guernsey characters.

The greatest value of the advanced register comes through its revelation of great producing cows and sires of the breed, and, through them, of the producing strains. It thus aids in the only way man probably has for permanently improving the breed, *i.e.*, by selection in breeding. There are, however, some weaknesses in the system. In the first place, the best cows are repeatedly retested for still higher records. Since the test period is twelve months, the period between consecutive calving is prolonged to such an extent as materially to decrease the offspring of the best cows. Furthermore, the rich feeding to which such cows are subjected probably interferes with the reproduc-

tive function with the same result. These are weaknesses which might be overcome to a large extent (1) by making the test period the same as the gestation period, (2) by establishing a herd ideal in breeding rather than an individual cow ideal, *i.e.*, by breeding for a "500-pound or 600-pound equivalent" herd rather than a 1200-pound cow. By stating records of young cows in terms of mature cow equivalents, a uniform herd standard can be established without the necessity for so much retesting which probably often obscures the real value of a cow as a breeding animal by involving the element of training in production.

CONCLUSIONS

By way of summarization it may be said:

(1) Marked prepotency is limited to a comparatively few animals;

(2) Strongly prepotent sires usually belong to prepotent families or strains;

(3) Prepotency is probably enhanced by inbreeding;

(4) Prepotency may become a valuable aid to intelligent selection in breeding for greater production.

Heredity and Juvenile Delinquency

The view of Dr. William Healy, director of the Juvenile Psychopathic Institute of Chicago, that heredity is only a minor factor in juvenile delinquency, is not shared by Dr. W. J. Hickson of the psychopathic laboratory of the municipal court in Chicago. In a recent interview, Dr. Hickson said: "After all these opportunities of learning and checking up, and the efforts of men of science to spread the information, we still see many people who would solve the delinquent boy problem by trying to 'change the leopard's spots' by environmental means.

"If the boy is normal, environment has little or no influence on him.

"Thanks to the laws of heredity, some of our greatest men have attained eminence despite the slums in which they were born and raised.

"The normal boy will take care of himself in any environment. That playgrounds, social centers and the like are good for him, no one will deny. That they are essential is not true, because normality means adaptability.

"These social agencies do not reach the defective, and cannot."

Dr. Hickson concludes that most juvenile delinquency is due to mental defect, the greatest cause of which is heredity. A campaign for "negative eugenics" would therefore be desirable.

Sale of Canadian Cattalos

The Canadian government has purchased twenty cattalos (hybrids between the American bison and domestic cow) from the estate of the late Mossom M. Boyd, of Bobcaygeon, Ontario, whose breeding experiment was described in the JOURNAL OF HEREDITY for May, 1915. Sixteen cows and four bulls made up the herd purchased, which was sent to Scott, Sask. The Boyd estate retained thirteen, three of which have been killed. The remaining ten will be bred in a continuation of the experiment, which aims to transfer to the domestic (Polled Angus) cattle the valuable hump and fur of the buffalo. The government herd will be bred along similar lines.

THE PITANGA

A Valuable Fruit of Brazil Which Deserves to be More Widely Cultivated— Successful in Florida and California— Methods of Propagation

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"IT IS said," writes Padre Tavares, "that the pitanga dropped from the hands of Nature, while she was at play one day, and became at once a charm to the eye and a delight to the palate. Each fruit is a glowing ruby, suspended by a delicate stem amidst the cool, green leaves of the pitanga tree, a challenge alike to the covetous eyes of children and of birds; nor can the decrepit old man, bowed down with the weight of years, escape its attractions, for he seats himself beneath its shade to meditate upon the Eternity which is approaching. Surely, Brazil does not have to envy Europe her cherry trees, bending in May under the weight of their ruby fruits; our pitangas surpass them both in beauty and in taste."

These lines from one of the ablest of living Brazilian naturalists testify to the esteem in which the pitanga is held throughout those parts of Brazil in which it is found, and indicate something of the beauty of this fruit—rare as yet in most other tropical and subtropical countries, but worthy of extensive cultivation.

During our visit to Brazil in 1913-14, made primarily for the purpose of studying the Navel orange in its native home, Bahia, we were particularly impressed with the value of two native fruits which were found abundantly in the coastal cities. One of these, the jaboticaba, was described in the July, 1914, issue of the JOURNAL OF HEREDITY. The other, which is the subject of this article, is better known outside of Brazil than the jaboticaba, but its cultivation is by no means so extensive as we believe its value warrants.

Following a stay in Rio de Janeiro, our expedition, consisting of P. H. Dorsett and the writers, moved to Bahia for the purpose of making a detailed study of the Bahian Navel orange, the parent variety of the Washington Navel. The results of this study have been described by the senior author in the JOURNAL OF HEREDITY for July, 1915. Of the many interesting plants found in connection with the culture of the Navel orange at Bahia, few were so conspicuous and none so interesting as the pitanga, *Eugenia uniflora* L., of the Myrtle family. Many of the roadways in Cabulla and other Navel orange districts of Bahia were found to be bordered on either side with beautiful and well kept hedges of this plant, as were the driveways or walks leading to the residences of many orange growers, which are usually set back some distance from the road. Frequently pitanga trees are grouped near the houses, furnishing an abundant supply of fruit for home use. The general cultivation of this plant among the orange groves of Bahia, both as an ornamental and for fruit production, suggested its possible use for similar purposes in the orange growing districts and warmer portions of the United States.

DESCRIPTION

The pitanga, as usually seen, is a broad, compact shrub, but occasionally it forms a slender trunk and becomes a small tree up to 30 or 35 feet high. Its foliage is deep green

A TROPICAL RIVAL OF THE CHERRY

Cherries fail to grow well in the tropics, but Brazil has a fruit which might largely take their place. This is the pitanga, a member of the Myrtle family, whose beautiful red berries possess an aromatic pungency almost unknown in the fruits of cooler climates. Photograph of fruit grown at Miami, Fla., natural size. (Fig. 9.)

and glossy, the new growth of a rich wine-color. The flowers, which are about half an inch broad, have four white petals, with a tuft of stamens in the center, and are delightfully fragrant. They are produced singly on slender stems about an inch long, and are followed by bright crimson fruits, of the shape of a small tomato, deeply ribbed, about an inch in diameter, with a thin skin and melting, juicy flesh of aromatic, spicy flavor, comparable to none of the temperate fruits but somewhat resembling other members of the Myrtaceae. The single large seed, about the size of a cherry stone, lies in the center of the fruit and is easily removed.

Technically, the species may be described as follows:

An arborescent shrub or small tree, commonly branching close to the ground, broad, compact, sometimes developing a trunk 1 to 2 m. in height, with an erect, rather broad crown. In Brazil it often attains a height of 6 or 8 m.; in Florida it does not usually exceed 4 or 5 m. Branchlets thin and somewhat wiry, glabrous. Bark light brown, very smooth.

Leaves subsessile, opposite, entire, reddish when young, emitting a pungent odor when crushed, the blades ovate, shortly acuminate at the apex, rounded to subcordate at the base, 3–5 cm. long, 2–3 cm. broad, glabrous, finely pellucid-punctate, deep green above, paler beneath, midrib slightly impressed above, midrib and the rather few transverse veins slightly raised below, the transverse veins uniting toward the margin; petiole not over 3 mm. long, slender, grooved above.

Flowers white, fragrant, solitary upon slender glabrous peduncles 1½–2½ cm. long in the axils of the leaves. Calyx tube cylindrical, the lobes pale green, oblong, concave, rounded at the tips, 4 mm. in length, ciliate, reflexed. Petals 4, oblong-obovate, concave, 8 mm. long, 4 mm. broad, ciliate. Stamens numerous, erect in a large cluster, the filaments 4-6 mm. long, white, filiform, the anthers oval, pale yellow. Style slightly longer than the stamens, filiform, the stigma simple. Ovary quadrilocular.

Fruit a berry, one or sometimes two-seeded, depressed-globose, 2–3 cm. in diameter, prominently eight-ribbed longitudinally, deep crimson, shining, crowned by the persistent green calyx lobes, the disk small, sunken. Epicarp thin, membranous; mesocarp soft, juicy, crimson, of pungent, subacid flavor. Seeds spherical when 1, hemispherical when 2, commonly under 1 cm. in diameter; seed coat membranous.

While generally known as *Eugenia uniflora* L., it has one or two synonyms which are occasionally seen. Chief among these is *E. Michelii* Lam., a name which was used in this country until a few years ago and is still commonly applied by some foreign writers. Nurserymen in Florida and California have sent out numbers of plants under this name. *Stenocalyx Michelii* Berg was used by Barbosa Rodrigues, one of the best known Brazilian botanists, and following him other Brazilian writers have used it. *Myrtus brasiliana* L. is an obsolete name occasionally seen in old works, while *Plinia rubra* L. and *Plinia pedunculata* L. are also listed in the synonymy of the species. Piso and Maregrav called it *Ibipitanga*.

DISTRIBUTION

The pitanga is indigenous in Brazil, extending over a wide area. Tavares[1] states that it is found in the states of Rio de Janeiro, Paraná, Santa Catharina, and Rio Grande do Sul, where it grows along the banks of water courses and rivers, and in the edges of the forest. It is common in cultivation throughout many other sections of the country. It is interesting to note that Thomas Green,[2] in 1823, listed the pitanga as a "native of Goa in the East Indies." Goa is a small Portuguese colony on the western coast of the Indian peninsula, and during the early days of Portuguese colonization there was an interchange of economic plants between this colony and the Portuguese possessions in America, resulting in the cashew (*Anacardium occidentale* L.), the guava (*Psidium guajava* L.), and other American plants becoming thoroughly established in India, while the mango, the carambola (*Averrhoa carambola* L.), the jak (*Artocarpus integrifolia* L.), and other oriental fruits were transferred to Brazil. Quite probbably the pitanga was carried to Goa along with other Brazilian plants, and Green took it to be indigenous. Many of the citrous fruits which were established in Brazil by the Portuguese may also have come from Goa by way of Portugal.

The name pitanga, by which this fruit seems universally to be known in Brazil, is undoubtedly of Tupí origin. The Tupí Indians inhabited a large part of Brazil at the time of the discovery, and the names which they gave to many indigenous plants have persisted to the present day, though their meanings have in many cases become obscure. Martius[3] states that pitanga

[1] Tavares, Prof. J. S., "As Fruteiras do Brazil," in *Broteria*, Vol. X, fasc. V, Braga, 1912.
[2] "Universal Herbal," Vol. I, London.
[3] Beiträge zur Ethnographie und Sprachenkunde Amerikas zumal Brasiliens, Vol. II, Leipsig, 1867.

is derived from the Tupí verb *piter*, to drink, and *anga*, odor or scent; one may assume, therefore, that the name was given in reference to the characteristically aromatic juice of the fruit.

In other countries the culture of the pitanga is generally very limited. No references to its culture at Goa have been found in recent literature, but we may presume that it is still grown in that region. It is cultivated in northern India at the botanic garden of Saharanpur, and is offered in the catalog of this institution under the name of "Brazil Cherry," but the statement is made that it does not fruit abundantly in that climate. It is grown in Ceylon, where, according to Macmillan, it is called Goraka-Jambo. Tavares states that it is cultivated in China, but we can find no other authority for this. It seems quite probable that it may have been introduced in the Portuguese colony of Macao, near Canton, where the climate would probably be very favorable to its growth. It is said by Wilder, who calls it the "French Cherry," to be a common garden plant in Hawaii. In French it is generally called cerise de Cayenne, or Cayenne-cherry; Dr. Trabut[4] states that it would rapidly become popular in Algeria if it produced more abundantly, having been found to be quite hardy along the coast. Emile Sauvaigo,[5] another French writer, states that it is the commonest tropical fruit cultivated in the vicinity of Algiers; he gives it the common name of cerise carrée as well as that applied by Trabut. In Cuba it is occasionally seen in gardens, and is called cereza de Cayena. In the United States its culture is limited to Florida and California, but it is grown very successfully in both these regions. In 1887 P. W. Reasoner[6] wrote, "The tree is quite frequently met with in Orange County and middle Florida, and is gaining in favor as a fruitbearing plant." At the present time it is common in gardens along the East coast, especially in the vicinity of Miami, where the fruit has recently commenced to appear in the market, and on the West coast from Fort Myers northward. After the plants have attained the requisite age they fruit abundantly, often producing two crops a year.

In California the pitanga has never become so common as in Florida. It is, in fact, rare in California gardens, and undoubtedly worthy of much more extensive cultivation. Dr. Franceschi[7] reported that it was growing in Montecito in 1895. In recent years quite a few plants have been disseminated by nurserymen in California. In the United States the name Surinam-cherry is much more common than pitanga, and is, in fact, the one generally used.

HABITS OF GROWTH

At Bahia, Brazil, the pitanga was found much more commonly as a hedge plant than in any other form, but such plants produce much less fruit than those allowed to develop naturally. The largest trees seen in Bahia, at the Roça Coronel in the suburb of Roma, were about 15 feet high, with trunks six inches in diameter. At Agua Comprida, near Bahia, on the ranch of Col. João de Teive e Argollo, we found a magnificent specimen nearly 35 feet high, with a trunk 15 inches in diameter. These trees, in December, which is one of the spring months in Bahia, were loaded with their bright red fruits. Considerable variation was noted on different trees, in regard to quantity, shape, size and color of fruit. Dr. V. A. Argollo Ferrão, to whom we are indebted for invaluable assistance during our stay in Bahia, stated that he had frequently observed this variability of individual trees, and suggested the possibility of improving the pitanga by careful selection of seeds. Bud selection would not be practicable at present, since seed propagation is the only method generally employed in Brazil. By planting seeds from trees

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[4] In *Revue Horticole de l'Algerie*, XII, p. 161, 1908.

[5] Les Cultures sur le Littoral de la Mediterranee, p. 207, Paris, 1913.

[6] Report on the Condition of Tropical and Semi-tropical Fruits in the United States in 1887, p. 25, Washington, 1891.

[7] *Santa Barbara Exotic Flora*, p. 33. Santa Barbara, Cal., 1895.

PITANGA HEDGES ALONG THE ROADWAY

This scene near Bahia, Brazil, shows one of the best uses of the pitanga—to form a hedge. But it is not limited to this use, for it makes a shapely tree, and bears even more heavily, when planted alone. In northern climates, it might be used as a pot plant, and produce its strikingly beautiful berries indoors. It is remarkable for the great speed with which it develops its fruit, which in the tropics is sometimes ripe within three weeks from the appearance of flowers. (Fig. 10.)

which bear abundantly, and whose fruits are of good size and quality, marked improvement could probably be secured. This has recently been illustrated in California with the feijoa (*Feijoa sellowiana* Berg), a plant belonging to the same family; seeds from selected fruits have produced much better results than those taken without regard to parentage.

Two varieties have been offered by Reasoner Brothers of Florida; one the ordinary crimson-fruited pitanga, and the other "black-fruited," being considerably darker in color than the common type. Tavares mentions two

varieties which occur in Brazil, but states that they appear equally good to him. There are so many variations among seedlings that it would undoubtedly be an easy matter to distinguish a number of horticultural varieties; it would be less easy, however, to insure their coming true from seed. When some readily applicable means of vegetative propagation has been found, more attention can profitably be given to this subject. Fortunately, there appears to be less variation among seedlings than occurs among many tree fruits which have been subjected to long cultivation, the pitanga being comparable in this respect to its near relative the strawberry guava (*Psidium cattleianum* Sabine.)

THE FRUIT AND ITS USES

Bahian pitangas were found usually to be slightly less than an inch in diameter, flattened, deeply ribbed, and commonly containing one seed with a thin, gray seed-coat which becomes papery when dry and is easily removed. The size of the seed varies greatly in fruits from the same tree and is not always the same in proportion to the size of the fruit. Several large fruits were found which had comparatively small seeds and a large amount of soft, juicy, spicy pulp.

Miss Thompson[8], who has recently made an analysis of pitanga fruits in Hawaii, finds that they contain a total of 9.30% solids, of which 1.93% are insoluble. The percentage of acids is 1.44, of protein 1.019, and the total percentage of sugars 6.06. Fat is present in about .6%.

The uses of the fruit are numerous. As a fresh fruit, when fully ripe, they are delicious, though sometimes the novice finds their strongly aromatic, almost pungent flavor peculiar and even disagreeable. The jelly which is made from them possesses a character all its own, and vies with guava jelly in popularity among Bahians. It impressed us as being a product of unusual merit. Pitanga sherbet is an especial favorite in Bahia, and is regularly served in all the cafés. It is of a beautiful deep salmon color, and delicious in flavor. A liquer is sometimes prepared from the fruit, and also syrups and wines which are considered by the Brazilians to have medicinal value, being stomachic and facilitating digestion.

Aside from the fruit itself, the foliage is extensively utilized by the Bahians, being highly esteemed for decorative purposes. In the notes made by one of the writers on Christmas Day, 1913, the following paragraph appears:

> "The people use pitanga branches to decorate carts, animals, street cars, and houses. The leaves are scattered over the floors of the living rooms in the houses, and when crushed under foot give off a delightful, refreshing, pungent aroma."

The use of this plant for decorative purposes at Christmas time is probably more extensive in Bahia than is the use of holly in the eastern United States; it seemed to be, in fact, the most popular decorative plant of the region. During the holiday season bunches of pitanga branches were offered for sale by vendors on almost every street.

In the United States, the fruit is usually eaten while fresh or is made into jelly. Pitanga sherbet should be tried by all who can obtain the fruits, however, and other uses will doubtless present themselves as the fruit becomes better known in this country.

THE CROP

A remarkable thing about the pitanga is the short time which elapses between the appearance of the flowers and the ripening of the fruits. Tavares assures us that the fruits are ripe within three weeks from the time of flowering, and in Florida, where the climate is not so tropical, they ripen within five or six weeks. In Brazil the plants bloom in September and ripen a small crop in October, flowering again for the main crop about December or January. In Florida the main crop is produced in March, with a few late fruits extending the season until May or June, and sometimes a second crop late in summer. In California the season is September or October.

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[8] "The Composition of Hawaiian Fruits and Nuts," in the Report of the Hawaii Agrl. Exp. Sta., 1914.

The plants are said by Bahians to fruit regularly, one grower estimating the number of fruits produced by an individual bush at 5,000, or sometimes more. In Florida they appear to fruit very regularly and abundantly, after they have attained sufficient age. Apparently it requires several years for the plants to come into full bearing, as they are of rather slow growth. It has frequently been reported in California that the plants do not fruit well. Probably this is due in many cases to lack of sufficient age, since old plants at both Santa Barbara and Orange have borne good crops. During the first four or five years little fruit seems to be produced.

PROPAGATION AND CULTURE

Thomas Green gave the following directions for germinating the seeds:

"Set the stones fresh from their places of natural growth in small pots filled with light earth, plunge them into a hot-bed, observing to keep the earth moist, but not wet. In about six weeks the plants will appear; when about 4 inches high, separate them very carefully, plant each in a small pot, plunge them into a hot-bed again, and carefully shade them until they have taken root. Treat them in the same way as other tender plants from the same countries, keeping them plunged in the tan-bed, and watering them sparingly in winter."

These directions have been followed by one of the writers at Riverside, Cal., and have given very satisfactory results. The seeds usually germinate well, if planted while fresh; at Reasoner Brothers' nurseries, Oneco, Fla., they are allowed to remain on the ground under the tree until they germinate, when the young plants are taken up and potted. It can thus be seen that there is no difficulty in starting the seeds, and they transplant readily.

Tavares states that the plant prefers a light, sandy soil. It grows well in south Florida on shallow, calcareous soils, and almost equally well in California on sandy loam. Unless trained, the plants usually assume a bushy, compact form, and branch close to the ground. They require no unusual care and seem to be fairly drought resistant, though coming from a moist region. The amount of frost which they will stand when young is not great, but they have passed successfully through temperatures of 27° or 28° above zero F., and when they have attained three or fours years' growth they should stand even lower temperatures without any injury whatever.

Genetics at Washington Experiment Station

The studies of inheritance at the Washington State Agricultural Experiment Station have been conducted with wheat, oats, barley and rye. The investigations with wheat include the qualitative characters and smut resistance. The inheritance of such specific characters as beards, head length and grain color, and the general characters of drought resistance, milling quality and stiffness of straw are among those which are being investigated. Many of the specific characters have been found to behave in a manner that can be definitely predicted when certain varieties are crossed. The inheritance of some of the more general characters remains to be determined.

The difference in the resistance of different varieties of wheat to smut has been determined. Various crosses are being made for the purpose of producing more valuable varieties with a less tendency to smut.

The studies with oats include the inheritance of panicle type, glume color, hullessness, etc. Similar studies are being made with barley. In the work with rye attempts are being made to obtain a variety without beards. Three generations or four crop seasons are necessary to determine the inheritance of specific characters of unknown value.

WAR, SCIENCE, CIVILIZATION

Biologist Protests Against the Kind of Biology Preached by Militarists—Defective Politics Resting on Defective Understanding of Nature Makes War Now Possible—Changes in Public Sentiment That Will Make War Unlikely—What the United States Could Do

Review of a book by WILLIAM E. RITTER
Director of the Scripps Institution for Biological Research of the University of California, La Jolla, Cal.

THE idea of settling by arbitration the question as to whether a hungry man may take a loaf of which he has the full strength to possess himself is chimerical and quixotic.

So says a recent German writer, attempting to justify wars of expansion. Taking this statement as a starting point, Professor William E. Ritter, of the University of California, has undertaken[1] to show what modern biology would say about war of that type.

In the first place, he strongly objects to the tendency of militaristic writers to justify wars among men on the mere ground of a struggle for survival among the lower animals. Admitting the truth of the statement made in the first paragraph of this review, he remarks that it does not cover the whole case.

"Such situations constitute what militarists of the Homer Lea and von Bernhardi type regard as the biological necessity for war. As a biologist, I would insist that the argument which would make war everlastingly necessary on such grounds implies a limitation to the conception of 'biological' that is utterly inadmissible by biology itself. Biology never stops and never can stop in its dealings with any animal by regarding it just as an animal in an unrestrained sense. It always deals with some particular *kind* or *species* of animal. The fish must be treated as a fish, and the bird as a bird. Neither can be disposed of by merely attending to such general attributes as need for food and propagation, common to both, and to all animals.

"In exactly the same way is it impossible for biology to consider man as just an animal. If it touches him at all it must touch him as the *human* animal. Confusion of thought in this matter, not only among laymen but among many biologists, is amazing, and has led to the most bizarre speculations about man, some of these being truly direful in their effects on human outlook and conduct."

If biology, then, is to be drawn into the discussion of war, it must insist that man be considered as distinctly a human animal, endowed with reason, and foresight, and inventive talent, and humane sentiments.

Given these endowments, man dehumanizes himself if he does not use them to forestall situations that would make hunger press so severely on him as to lead him to war. This is a question of the proper distribution of the earth's resources.

THE HEART OF THE PROBLEM

"This brings us to the kernel of this discussion, and, as it seems to the writer, to the supreme question our nation will have to grapple with if it would accomplish anything significant toward world peace. That question is, Can we present any practical plan whereby nations foremost in the march of civilization shall be assured such

[1] War, Science and Civilization, by William E. Ritter. Pp. 125; price, $1 net. Boston, Sherman French & Co., 1915.

portions of the primal resources of nature as are necessary to enable them to maintain the places they have won, without having to resort to war to secure them?"

"Stating the matter still more pointedly, is an international arrangement possible whereby a nation might under certain circumstances give over to other nations portions of its territory or other economic advantages peacefully, deliberately, and without immediate and definite compensation? The suggestion even in the form of a question will probably seem too absurd to merit a moment's thought by practical men. My own categorical answer to the question is, no, as long as politics, national and international, rest on a philosophy of nature and human nature so defective as that on which they now do rest; but yes, if political practice could be based on a philosophy that should conform to the actual facts of nature and human nature."

Politics can never be scientific, Dr. Ritter thinks, because it is devoted to the meeting of exigencies, the dealing with matters of expediency, which are quite alien to the spirit of science. But if politics can not be a science in a strict sense, yet it cannot measure up to the real needs of modern civilization, unless it rests on a foundation a large part of which is science.

An essential part of this foundation would be the recognition of those general principles of nature and human nature upon which man, the human animal, would base his efforts to ward off in effective fashion crises of national want, and thereby avoid being placed in the position of the hungry man who takes by force the loaf of bread.

WAR DEFEATS OWN END

From this point of view, it is of first importance to recognize that war defeats its own end. It is anomalous that a system of distributing the necessities of men's existence among the political divisions of the earth, should be in vogue whereby in order that men may get that which they must have, they are obliged to destroy a large portion of that for which they are striving. Those who defend war as a means of gaining territory or other economic advantage, and refer to the biological struggle for existence as a justification, forget the nature of the sub-human struggle. That results in the destruction or defeat of some of the combatants merely; while the struggle among human beings, especially those living under civilization, results in destroying not only some of the combatants, but much of the goods over which they fight. From this standpoint, so-called civilized warfare is far less scientific than the pillaging warfare among savages, which aims chiefly at capturing and carrying off the goods for which it is waged.

Further, when politics invokes the support of biology to justify war, it must recognize that "Nature's resources are actually limited for partly civilized man, but potentially unlimited for fully civilized man." So far as nature and science are concerned, there is ample reason to believe that civilization might ensure its own progress indefinitely, even though "pressure of population upon means of subsistence" be accepted as an inevitable concomitant of that progress. But an essential condition of continued progress would be the utilization of all the resources of nature to the fullest extent.

In the way of doing this stands the stupendous obstacle of existing political ideas and practices relative to the ownership of its primal resources. "It seems unescapable that if science is to be enabled to do its best for civilization, some way will have to be found to overcome this difficulty. Nothing could be further from scientific than the way Africa and the Pacific islands are being allotted among the civilized nations. Perhaps there is little hope of early reaching a rational basis in this matter. Surely there would be none were it not for the fact that civilized men are ruled so largely by general theories held in the blindest way; but that these theories may undergo profound change when personal interests are seen to be at stake; and that, on the whole, right theories appeal more to normal men than wrong ones."

All of the combatants in the present world war believe that they are fighting for the cause of civilization; but they do not define what they mean by civilization, and it is doubtful whether they really know. To the author, Civilization "is the one-word designation of the grade of evolution for one organic species, *Homo sapiens*, in respect to those attributes which set it off most sharply from all other species. It *is* evolution, though only a part of it, albeit an overwhelmingly important part."

Now civilization in this sense, the author goes on to show, is incompatible with great empire. "More disastrously fallacious reasoning was never carried on than that according to which a nation's status in civilization is dependent upon its territorial and economic extent. . . . The reasoning that would justify strife for unlimited possessions just for the sake of having them, would be paralleled by reasoning that because the individual cannot live without food, therefore he should try to eat all the food in sight."

The nation which wishes to achieve a high degree of civilization, then, cannot spend its energy squabbling over boundary lines, but must devote itself to developing civilizing processes. It must recognize that science furnishes the groundwork for a great rational faith in man's capacity for indefinite progress. It must recognize that the phenomena of cooperation, coordination or, as the author prefers to say, integration, are just as much a part of evolution as are the phenomena of differentiation, which we usually have in mind when we think of evolution. The militarists who are seeking shelter behind biology commonly ignore this, and speak of a "right of conquest" as something sacred, because it has existed in the past.

"Such arguing is intolerable to a consistent evolutionist. In the use made of the doctrine by political leaders, diplomatists, and militarists, the utmost contradictoriness and confusion prevail. On the one hand they borrow from biology and use with the greatest assurance such vague phrases as 'struggle for existence' and 'survival of the fittest,' while on the other hand they seem quite oblivious to the essential idea of forward movement and growing interdependence among men, the very essence of progress in civilization."

"From the standpoint of biological evolution, progress in civilization may be characterized as the differentiation and intensification of love and intellect, and of the intellectualizing of love and the affectionizing of intellect."

A CHANGE IN POLITICS

Now the author inquires, if such a view of human nature were generally adopted, as being based on scientific grounds, and if man should at the same time get "the mighty faith that there is practically no limit to nature's capacity for yielding to man all those things which, from sources outside himself, he truly needs," what would be the psychological result? What would be the effect on the attitude and conduct of men toward one another and toward nature?

On the negative side, it would banish the dread of the "tragedy of population," which has been present ever since Malthus created it.

On the positive side, it would imbue productive effort with a religious zeal. The tasks of conserving, developing, distributing, and wisely using the forces of nature, would be viewed in a truer light. Men would really gain a religious feeling, which would direct them in subjugating nature, rather than in subjugating men and nations.

"It remains to ask what our nation might do at this time to forward this great end. Manifestly we cannot escape playing some part in the grim world-drama now being staged. The answer may be short and sharp. Two sorts of things may be done; indeed, must be done, if the part we play is to be positive and honorable. One sort will pertain to the nation itself; the other to its relations with other nations."

The first thing to do is to "subject ourselves to a self-examination the like of which we have hitherto known little about." It will result in the increase and improvement of our education,

culture, and the pursuit of art and science.

The second is to put into practice the principles which underlie civilization, by showing other governments that we are willing to help them get what they need for their own greatest development. As a hypothetical case, Dr. Ritter suggests that we might turn over to Japan some of our non-contiguous possessions, in order that her congested population may have room to breathe.

"The possibility of conditions in which the policy of England would be to help Russia to better seaports, if Russia truly needs them; of France to help Germany to more and better room in Africa for colonization, if Germany's needs in that direction are clear; and of Germany to help Japan, the United States, and Great Britain to free the whole Pacific from need of extensive armaments, might be counted on to fill millions of persons the world over . . . with an enthusiasm that would be irresistible and permanent because sustained by reason as well as by emotion.

"This is idealism, but it is scientific idealism."

The "Practical Eugenic Movement"

The "Practical Eugenic Movement," an organization directed by T. W. Shannon, of Delaware, Ohio, has more than 7,250 members, according to a recent letter from Professor Shannon. It publishes a monthly magazine called *Practical Eugenics*, which is said to have more than 4,000 circulation. The movement and organ are devoted largely to euthenics, emphasizing such factors as sex hygiene, temperance, care of the baby, personal hygiene and a war on tobacco, and a widespread propaganda is carried on through lectures.

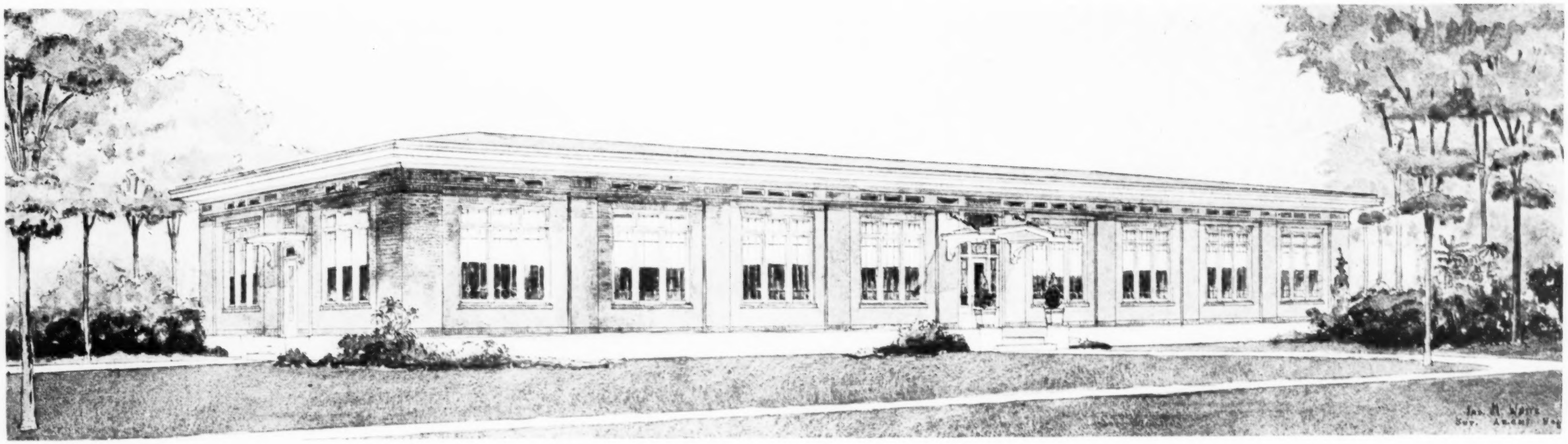
New Publication on Genetics

The first issue of *Genetics*, a bimonthly periodical record of investigations bearing on heredity and variation, appeared at the end of February. It contains as frontispiece a hitherto unpublished portrait of Gregor Mendel, and the following papers: "Non-disjunction as proof of the chromosome theory of heredity," by Calvin B. Bridges; "The numerical results of diverse systems of breeding," by H. S. Jennings; and "Hereditary anchylosis of the proximal phalangeal joints (symphalangism)," by Harvey Cushing. *Genetics* is edited by a board, of which Prof. George H. Shull, of Princeton is the chief, and is published by the Princeton University Press. It is announced that it starts with about 270 subscribers, who pay $6.00 each per year.

Rare Publications on Genetics Available

Through early members, the American Genetic Association has come into possession of several complete sets of the Proceedings and magazine of the American Breeders' Association, and the volumes of the JOURNAL OF HEREDITY previous to the present year. These entire sets of the Association's publications are offered for sale as wholes, and the secretary will be glad to correspond with any one interested in securing a collection of these publications.

DEPARTMENT OF GENETICS AT ILLINOIS COLLEGE OF AGRICULTURE



The Division of Genetics in the College of Agriculture at the University of Illinois has recently been removed to its new building. The building is 142 feet long and 42 feet wide, and contains two animal rooms, a storeroom, a graduate laboratory, class room, seminar room, and two offices. The work in Genetics was begun by Dean E. Davenport many years ago and has grown steadily since. Four years ago Dr. J. A. Detlefsen took charge of the courses and investigations. The College of Agriculture at the University of Illinois was among the first of the Agricultural Colleges to recognize the importance of Genetics to the curriculum of well-trained agricultural students. An elementary course in Genetics has an enrollment of 300 students; an advanced course enrolls about fifty seniors and graduate students. Research upon small laboratory mammals is in progress. Numerous species crosses in lettuce have been studied. A study of the transmission of mutations in the skunk has been carried through the first cross-bred generation. Four distinct types of mutants have been obtained, at least two of which promise a new and valuable type of skunk fur. (Fig. 11.)

THE STRAWBERRY, A TRIUMPH OF PLANT BREEDING

IF one asks, "What has deliberate, systematic plant breeding accomplished in horticulture during recent years?" no general answer can be given. In some fruits scientific plant breeding is still almost unknown. The varieties of cherry, for example, which are grown in the United States are practically all of unknown origin; they appear by accident and some one recognizes their merit and propagates them. In other fruits much more has been done by scientific breeding, but probably with no single fruit has more been accomplished than with the strawberry.

Of the 200,000 acres or thereabouts which are planted to strawberries in the United States, it is probable that 90% are planted to varieties which have been produced during the last quarter of a century, as the result, not of chance, but of intelligent manipulation by breeders.

Strawberries have been grown in this country ever since it was first settled, but the original strains bore fruit only in the early summer. One of the greatest advances in the industry was the introduction of the fall-bearing or ever-bearing varieties, the first of which was Pan-American, produced by Samuel Cooper, of Delevan, N. Y., in 1898. This was a sport from the variety Bismarck, which bore only in the early summer.

Mr. Cooper produced a number of seedlings by self-fertilization from this sport, and then crossed them with each other and back on the parent. In this way a number of other more desirable varieties were obtained.

Crossing Pan-American with Dunlap (Senator Dunlap), a widespread and famous variety, Harlow Rockhill, of Conrad, Iowa, produced in 1908 the variety Progressive, which has proved one of the most popular in the northern States. The number of plants of Progressive in existence at the present time cannot be much short of 700,000,000 or 800,000,000—a pretty good record for eight years.

Dunlap itself, which has long been the most widely grown northern variety, originated with Rev. J. R. Reasoner, of Urbana, Ill., in 1890, but was not introduced to the trade until 1900. It was the result of definite breeding.

The variety Klondike, which makes up probably nine-tenths of the area planted in the southern States, was produced as long ago as 1895 by Robert Cloud, of Louisiana, as a result of a carefully planned cross.

Why, it may be asked, has intelligent plant breeding been so much more widespread with the strawberry than with any other fruit? The ease with which results are got appears to be the principal reason. Crosses are easily made, seedlings are easily grown, and they multiply so rapidly by runners that a large stock can be obtained in a very short time. Thus a commercial breeder, if he can produce something really good and keep it under his control for a few years, is able to offer for sale a large enough stock to bring generous financial returns. This seems to be the principal reason why the strawberry grower can get varieties produced with a view to meeting his definite needs, while the grower of many another fruit is forced to content himself with varieties that appeared by accident, and may be far from ideal.

Encouraging Race Suicide

"Some day," says the March *Eugenical News*, "it may be regarded as a crime against society to publish such an advertisement as the following which has been running in the *Survey*: 'Wanted — Married couples without children, between the ages of 30 and 45, to take charge of cottages' at a school for boys." But not if they prefer caretakers of inferior quality.

WHAT GENETICS IS

"An exact determination of the laws of heredity," says William Bateson, "will probably work more change in man's outlook on the world, and in his power over nature, than any other advance in natural knowledge that can be clearly foreseen."

To gain this knowledge is the object of the science of genetics, which proceeds, in practice, largely by means of plant breeding and animal breeding, for the reason that heredity is less complicated in these organisms than in Man, and its operation can be more easily made out. The knowledge so gained finds its application in methods for the improvement of cultivated plants and domesticated animals and, most important of all, in the improvement of the human race through the science of eugenics, which was defined by its founder, Francis Galton, as "the study of agencies under social control that may improve or impair the racial qualities of future generations, either physically or mentally."

THE AMERICAN GENETIC ASSOCIATION

is an incorporated organization, co-operative in nature and devoted to promoting knowledge of the laws of heredity in the broadest sense of the word, and their application to the improvement of plants, animals, and human racial stocks. It is the largest organization in the world for the advancement of genetics, and its organ, The Journal of Heredity, is the most important agent for furthering the interchange of ideas between investigators of different phases of these problems, and for the presentation of their results to the public.

Through its committees on research, co-operation with which is not obligatory, but is urged upon every member, the association collects information by means of approved scientific methods applied to the subject of genetics. This information, as well as that derived from other authoritative sources, it endeavors to place before the public by means of its committee on education and extension, and before its membership, in an attractive and understandable way, through this magazine. The association constantly strives to further the cause of conservative, constructive science and to check the progress of fallacious and sensational pseudo-science. While it can not assume responsibility for the accuracy of statements made by contributors to The Journal of Heredity, it endeavors to publish only such as are on a sound scientific basis, and members are urged to contribute such articles, with illustrations. The magazine does not pretend entirely to cover the immense field of genetics, but it is designed to keep members informed of the latest results in research in the most interesting lines, and to present these results in such a way that they will appeal not only to the specialists, but to the general reader who desires to know what the specialists are doing in a science that is of such personal importance to each individual.

REQUIREMENTS FOR MEMBERSHIP

Membership is composed of scientists, teachers, publicists, physicians, clergymen, students, horticulturists, and breeders of live stock, throughout the world. Subject to the approval of the council, any person interested in the improvement of the human race or the creation of better varieties of plants and animals, is eligible for membership. The secretary will be glad to correspond with those interested, and to send a copy of the magazine for examination. Annual dues, giving the right to attend all meetings and to receive the Journal of Heredity, are $2; life membership is $50. Address all communications to

THE AMERICAN GENETIC ASSOCIATION

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